THE MONTHLY MICROSCOPICAL JOURNAL:

TRANSACTIONS

OF THE

ROYAL MICROSCOPICAL SOCIETY,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

HENRY LAWSON, M.D., M.R.C.P., F.R.M.S.,

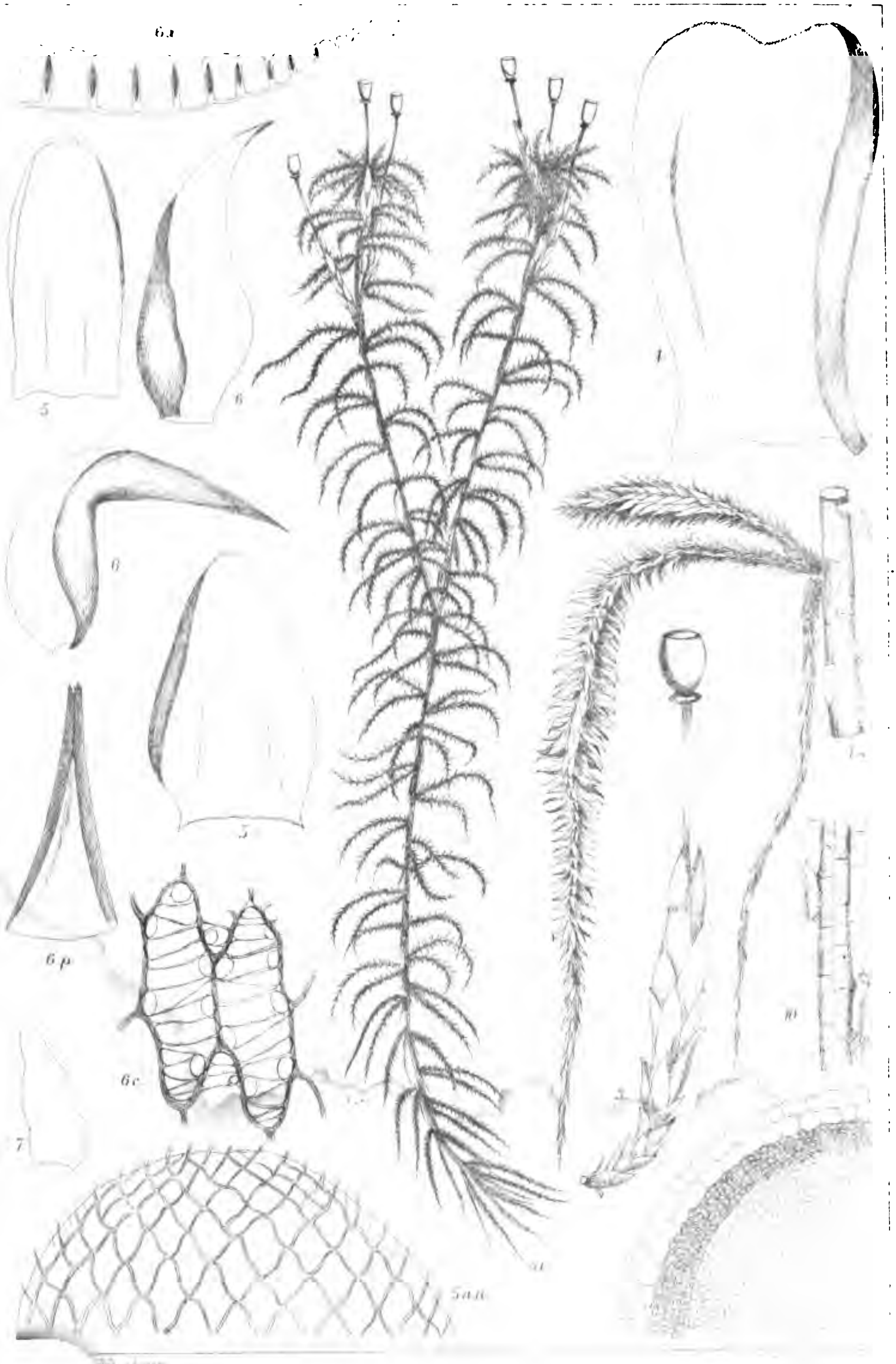
Assistant Physician to, and Lecturer on Physiology in, St. Mary's Hospital.

VOLUME XII.

LONDON:

ROBERT HARDWICKE, 192, PICCADILLY, W.

MDCCCLXXIV.


6a
5
6
1
6
5
6p
6c
7
5aa
del. ad nat.

THE

MONTHLY MICROSCOPICAL JOURNAL.

JULY 1, 1874.

I.—*Synopsis of the Principal Facts elicited from a series of Microscopical Researches upon the Nervous Tissues.*

By Dr. H. D. Schmidt, of New Orleans, La.

(*Read before the* Royal Microscopical Society, *June* 3, 1874.)

1. The *ganglionic bodies* of the spinal marrow and brain represent *plexuses* of *nervous fibrillæ,* which are continuous with the fibrillæ of the processes and axis cylinders arising from them. Each plexus, thus formed, embraces a *nucleus* which is distinguished by a double contour representing its wall; it contains a *large nucleolus* and a great number of small granules. The nucleolus is also distinguished by a fine, sharply-defined double contour, and filled up by granules. In the large ganglionic bodies of the spinal marrow, it contains besides the granules two clear bodies of a reddish lustre; the one of these is never wanting, and is distinguished by its size, amounting to $\frac{1}{600}$ mm. in diameter, and also by its brightness. It appears in the form of a vesicle with a dark granule in its centre. The other is usually somewhat smaller, and frequently absent. In many cases, in addition to the nucleolus, a few smaller ones are observed, which, however, contain no glimmering vesicles, but are only filled with granules.

2. *The Cortical Layer of the Cerebrum.**—The nervous portion of this consists of numerous ganglionic bodies of different form and size, and of vertically or horizontally-running nerve fibres arising from them; further, of a fine granular substance, lodging a *network* of fine *granular fibrillæ,* and also a considerable number of free nuclei. The typical form of the greater portion of the ganglionic bodies in this portion of the brain resembles a more or less spindle-shaped tuber, from the sides of which a number of greater or smaller conical processes arise; the whole body appears, therefore, in a thin section in the form of a triangle or pyramid. The average number of the processes is four or five. One of them, the *pyramidal* or *pointed* process, takes a vertical course toward the surface of the cerebrum; another, sometimes two, the *lateral* processes, pass in a more or less horizontal direction; and the rest, the *basal* processes,

* The description relates mainly to the convolutions of the convexity of the hemispheres of the cerebrum of man.

pass vertically toward the white substance. The pointed process attains, according to the size of its ganglionic body, a length of from $\frac{10}{100}$ to $\frac{25}{100}$ mm. In its more or less serpentine course it gives off a number of small lateral branches, which soon terminate in the above-mentioned fibrillous nervous network of the granular substance. I have never seen this process terminate in a dark-bordered nerve fibre. From the lateral process, a dark-bordered nerve fibre always arises; it attains a considerable length, and its course is more or less horizontal. A direct communication between two ganglionic bodies by means of these fibres I have never seen; in some cases, however, I have observed them dividing into two branches, the ramifications of which ended in the terminal network. From the basal processes, the nerve fibres of the white substance arise. On ganglionic bodies of medium size, two of these processes are ordinarily seen, one of which is converted into a nerve fibre, while the other divides dichotomously. One of the branches resulting from this division forms also the axis cylinder of a nerve fibre, while the other subdivides into finer branches, which terminate in the network. In the larger ganglionic bodies, even the first basal process divides and gives rise to two axis cylinders.

In the upper strata of the cortical layer a considerable number of smaller ganglionic bodies of a more triangular or quadrilateral form are met with. Their delicate processes run about in the same direction, and terminate in the same manner as those of the larger bodies just mentioned.

The nerve fibres of the white substance, arising from the basal processes, leave the grey matter in the form of bundles, composed of about eight to ten, or even more nerve fibres of different thickness. Generally, two or three of the fibres, of from $\frac{2}{600}$ to $\frac{4}{600}$ mm. in diameter are met with in each bundle; the rest are finer fibres, of about $\frac{1}{600}$ to $\frac{1}{500}$ mm. in diameter. One portion of the finer fibres appears to arise from the smaller ganglionic bodies of the upper strata, but another arises *directly* from the terminal nervous network. At first, the bundles of nerve fibres are separated from each other by considerable interspaces; but as others arise from the ganglionic bodies situated below, the interspaces become more narrow, until, at the border of the white substance, they are entirely lost, so that the bundles come to lay contiguous to each other.

In tracing the different nervous elements, imbedded in the granular substance of the cortical layer of the cerebrum, from the surface toward the white substance, we first meet with the exceedingly fine, felt-like, fibrillous neuroglia, covering the surface and extending throughout the whole cortical layer to the white substance. Directly under the neuroglia, in the uppermost stratum of the cortical layer, a considerable number of fine dark-bordered

nerve fibres are seen crossing in various directions; the terminal ramifications of which form an imperfect network. The meshes of this network, measuring about $\frac{1}{200}$ mm. in width, extend into the granular substance to a depth of $\frac{1}{60}$ mm., and are gradually lost in the fine terminal nervous network, already mentioned. A considerable number of free nuclei, surrounded by pigment granules, are distributed throughout the stratum to a depth of $\frac{5}{100}$ mm. Advancing farther, we now meet with the first ganglionic bodies, forming a layer of about $\frac{35}{100}$ mm. in depth. The direction of the processes of these bodies is various and indefinite, as some pursue a vertical and oblique course, while others run in an opposite direction. Their further connection is difficult to discover, as they can only exceptionally be traced to a greater distance than $\frac{2}{100}$ mm. From certain observations, I suppose that those fine dark-bordered nerve fibres on the surface, above mentioned, as well as others of the same kind, running horizontally in the interior, are derived from them. I doubt not but that these small ganglionic bodies also send a nerve fibre to the white substance. The transition of the ramifications of some of their processes into the terminal network can always be seen.

About $\frac{40}{100}$ mm. from the surface of the cortical layer, the ganglionic bodies commence to change their form by the lengthening of their pointed process, and thus approach the above-described pyramidal type. Gradually increasing in size, they attain, about $\frac{20}{100}$ mm. deeper, their maximum, with a length of from $\frac{7}{100}$ to $\frac{9}{100}$ mm. These bodies, the most perfectly developed, and representing the type, form a layer of about $\frac{90}{100}$ mm. in thickness. Advancing still deeper, they diminish again in size, and become, to a certain degree, though not without exception, more spindle-shaped, in which form they extend to the white substance. At the same time they decrease in number, so that in the lowermost layer of the cortical substance the fibrous element already predominates. The decrease in size of the ganglionic bodies in approaching the white substance, however, is not so considerable as might be supposed, for they still attain a length of about $\frac{6}{100}$ mm. or more; their pointed process, however, is proportionately thinner.

3. *The Cortical Layer of the Cerebellum.*—This consists of the true *cortex* or so-called *grey layer*, and of the reddish-grey *nucleated layer*. In the latter, the transition of the grey matter into the white, composed of nerve fibres, takes place.

The *grey layer* is about $\frac{49}{100}$ mm. thick, and consists of ganglionic bodies and free nuclei, imbedded in a fine granular substance through which a *terminal nervous network* extends. The principal ganglionic bodies are those of *Purkinje*, well known by their peculiar form. They are found at the inferior margin of the grey layer, whence they extend their enormous antler-shaped processes with

their extensive ramifications throughout the whole grey layer, to terminate in its terminal network. Another much smaller process, arising from a point opposite to the large process, passes into the nucleated layer, and is transformed into a dark-bordered nerve fibre. Distributed throughout the granular substance between the extensive ramifications of the large ganglionic bodies of *Purkinje*, a number of smaller ones of a triangular form is observed. From their acutely projecting angles fine processes arise, which, however, even in the most favourable cases, can be traced only to a very short distance. Single free nuclei are also distributed throughout the whole layer. In addition to the elements of the grey layer just mentioned, there are a considerable number of fine nervous fibrillæ, which, arising *directly* from the terminal network, pass over to the nucleated layer, either singly or in the form of anastomoses. They arise in all parts of the grey layer, and approach in a vertical direction its inner border. At the summit of the convolution they penetrate in this direction into the neighbouring nucleated layer, more or less parallel to the axis of the convolution. At the sides of the latter, however, they make a greater or smaller curve, before they leave the grey layer, in order to pursue a course parallel to the axis.

The *nucleated layer* is a continuation of the grey layer. Its examination is rendered difficult by a large number of nuclei densely distributed through it, and preventing one from obtaining a clear view of the relative arrangement of the axis cylinders and nerve fibres passing through the interspaces. In examining a very thin section at the summit of a convolution, it will be observed that the granular substance of the grey layer extends through the whole nucleated layer and even farther, and fills up the interspaces left between the nuclei, fibrillæ, and dark-bordered nerve fibres. There will also be observed a number of fine nervous fibrillæ, which, singly crossing or anastomosing with each other, pass over from the former to the latter. The greater portion of these fibrillæ are most probably derived from the terminal network of the grey layer; a small part of them, however, may arise from the smaller ganglionic bodies, a conjecture which I have not been able to confirm by direct observation. But it is certain that a large part of them originate in the network, not only in the grey but also in the nucleated layer. In extremely thin, somewhat torn sections, especially at the border of the nucleated layer, the fibrillous terminal network, passing through the granular substance, can be distinctly recognized, as well as fine and short fibrillæ arising from it, which, after anastomosing with each other here and there, finally unite to form fine axis cylinders. These are soon transformed into dark-bordered nerve fibres, and after passing through the nucleated layer, more or less parallel with the axis of the convolution, at last disappear in the white substance.

At the sides of the convolution, a part of the fibrillæ, originating in the network of the grey layer, take another course, by making, before leaving the grey layer a turn toward the base of the convolution; then they enter the nucleated layer, and, after being converted into dark-bordered nerve fibres, run along its border around the bottom of the sulcus, to pass over into the neighbouring convolution. These fibres connect, therefore, the grey layer of two convolutions. Nevertheless, other fibres, leaving the grey layer around the bottom of the sulcus, are also observed, which cross those connecting fibres obliquely and pass through the nucleated layer into the white substance.

The ganglionic bodies of *Purkinje* are found, as we know, near the inner border of the grey layer; many of them project even with a part of their body into the nucleated layer; their basal process belongs therefore to the latter. When we meet with this process entirely isolated, it is generally partly torn off, and therefore seldom longer than $\frac{1}{30}$ mm. Nevertheless, I saw it pass into a dark-bordered nerve fibre, which I was able to pursue, while passing along between the nuclei, to a considerable distance, and in a direction toward the white substance. Its diameter near its origin is $\frac{1}{300}$ mm., diminishing at a distance of about $\frac{1}{30}$ mm. from the body to $\frac{1}{600}$ mm.

The dark-bordered nerve fibres, issuing from the nucleated layer, form the white substance. The transition from the former to the latter, however, is only gradual, and it is therefore difficult to draw a distinct limit between them. The difficulty consists in the extension into the white substance, not only of the nuclei, lying now farther distant from each other, but also of the granular substance, which here still appears in the form of small isolated groups of granules, though without terminal network. The fine fibrillous neuroglia, extending itself between the nerve fibres, from the white substance below upward, contributes also to lend to the whole structure a confused appearance. As far as I have been able to ascertain, there exists no communication between the individual nerve fibres of the white substance of a group or leaf of convolutions of the cerebellum.* In examining these fibres in a thin section, or in a fine bundle, split off from the white substance at a point where they leave a group of convolutions, a considerable difference is found to exist in their diameter. While this, namely, measures in the greater number of fibres from $\frac{1}{400}$ to $\frac{1}{300}$ mm., others are met with of only $\frac{1}{800}$ mm., and, again, some even as thick as $\frac{1}{200}$ mm. We might venture to presume, therefore, that while the larger nerve fibres of the white substance be a continuation of the *basal* process of the ganglionic bodies of *Purkinje*, the axis cylin-

* The above description relates to the long convolutions of the hemispheres of the cerebellum of man.

ders of the finer nerve fibres are probably formed by the *fibrillæ* originating in the *terminal network;* some of them may also arise from the small triangular ganglionic bodies of the grey layer.

4. *The Ganglionic Bodies of the Sympathetic Ganglia.*—These are especially distinguished from those of the spinal marrow and brain by being enclosed in a membraniform capsule with which they are in a certain manner connected. From the body, enclosed within the capsule, a number of larger and smaller processes arise, consisting, like those proceeding from the ganglionic bodies of the spinal marrow and brain, of fine granular fibrillæ. These, however, cannot be distinctly traced over the body from one process into the other as in the former case; on the contrary, the whole body appears more as a mass of granules surrounding the nucleus.

In selecting a sympathetic ganglionic body of the *gangliated cords* of man as a type, we observe from two to four of the *larger processes*, directly after arising from the body, piercing the capsule and disappearing, at a distance of about $\frac{6}{100}$ mm. or more, in the form of naked axis cylinders among the neighbouring bundles of sympathetic nerve fibres. The two largest processes frequently arise from opposite points of the body; in some cases, the larger one of these, again, divides already dichotomously within the capsule, so that the axis cylinders arising from this division subsequently pierce the capsule. The axis cylinders arising from the processes possess a sheath manifesting itself by a double contour, and which, in many instances, may be traced back over the body; the diameter of the axis cylinders measures about $\frac{1}{500}$ mm.

The *smaller processes*, arising from the body, are more numerous than those just mentioned, and consist mostly of only two or even one fibril. After a course of $\frac{2}{200}$ to $\frac{3}{200}$ mm. alongside of the body, they enter the capsule at its inner surface, and form, by means of ramification and reciprocal connection, a *network* extending throughout this membrane; the interspaces of the network are filled up by small granules. The capsule of the sympathetic ganglionic bodies, therefore, represents *complicated, membraniform, nervous structure*, derived from and connected with the body which it encloses. On the surface of the capsule, formed in this manner, a number of fine fibrillæ arise from the network, a part of which pass, in the form of a finely reticulated plexus, over into the capsules of neighbouring ganglionic bodies, and thus establish a reciprocal communication; the rest surrounds the axis cylinders arising from the larger processes which have pierced the capsule, and, running with these in the same direction, unite among themselves to form finally the so-called sympathetic nerve fibres.

Scattered over the inner as well as the outer surface of the capsule, a considerable number of round or oval nuclei are observed. They are especially numerous in the reticulated fibrillous plexus

connecting the ganglionic bodies with each other, whence they extend, while assuming a more oblong form, between the sympathetic nerve fibres.

The destination of those axis cylinders, arising from the processes piercing the capsule, I have never been able to determine satisfactorily. Several times I have seen them isolated to a length of $\frac{3}{14}$ mm., and frequently I have traced them in thin transparent sections to a considerable distance, without noticing any change in their structure or even their diameter; they always disappeared between the bundles of sympathetic nerve fibres. Although these axis cylinders run mostly, as already mentioned, parallel with the sympathetic fibres, they nevertheless are observed to pass here and there through the latter in an oblique direction, and as it appears, toward the dark-bordered nerve fibres. In consequence of this fact, I presume that, after a shorter or longer course, they are surrounded by a covering and transformed into *dark-bordered* nerve fibres. If this conjecture should prove to be true, as we shall see farther on, the sympathetic ganglionic bodies would then give rise to both kinds of nerve fibres, each of which might transmit a nervous current of its own.

The ganglionic bodies of the *spinal ganglia* show, with some deviations, the same structure as those of the gangliated cords. There are certain difficulties, however, in the way of examining them accurately, produced by their more considerable size and a greater thickness of their capsule, as well as by a number of dark-bordered nerve fibres running between them. From the body within the capsule, as in the former case, a number of processes of different diameters arise; some of them seem to pierce the capsule, while the rest contribute to the formation of the capsule. The construction of the latter, as also the reciprocal connection with the neighbouring capsules, are the same as those of the ganglionic bodies of the gangliated cords, with this difference, that the meshes of the network are, in proportion to the greater size of the whole body, somewhat larger. The most essential difference, however, consists in a *dark-bordered* nerve fibre, originating directly in the spinal ganglionic body. The axis cylinder of this nerve fibre arises, as usual, from the body, and pierces the capsule, while the tubular membrane seems to terminate in the latter. Whether this dark-bordered nerve fibre connects the sympathetic ganglionic body with the spinal marrow, or whether it runs in an opposite direction toward the periphery, is difficult to determine, especially, as it first winds its way along for some distance between the neighbouring ganglionic bodies before it joins the bundle of nerve fibres. But judging from certain observations I made, I am inclined to think that it belongs to the spinal marrow. A part of the fibrillæ arising from the network on the outer surface of the capsule go to contri-

bute to the formation of the sympathetic nerve fibres of the nearest bundle.

The construction of the sympathetic ganglionic bodies is most distinctly seen in those of the *plexus gangliformis* of the pneumogastric nerve. Still larger in circumference than those of the spinal ganglia, their capsule shows a coarser and unmistakable network, the meshes of which attain a diameter of about $\frac{1}{150}$ mm. or more. The ganglionic bodies which lie, mostly in the form of oblong groups, between the dark-bordered fibres of the nerve, are here also connected with each other by the reticulated plexus of their capsules. Thus far I have not succeeded in discovering on these bodies more than one process piercing the capsule. This arises generally in the long axis of the nerve, and is, after a course of about $\frac{1}{10}$ mm. or more, transformed into a dark-bordered nerve fibre. As in the sympathetic ganglionic bodies before described, a considerable number of fine fibrillæ arise here also from the network on the outer surface of the capsule, a portion of which surround the axis cylinder, and unite finally to form sympathetic nerve fibres, while the rest, in the form of a plexus, establishes a communication with the adjoining capsules.

In reviewing the above-described construction of the sympathetic ganglionic bodies, it will at first appear more complicated than that of those of the central organs. By a closer examination, however, this seeming complication disappears, and a certain analogy of their component parts, especially with those of the cortical layer of the brain, may be recognized. The ganglionic bodies of the cortical layer of the cerebrum, namely, send out some processes, the lateral and basal, the axis cylinders of which are transformed into dark-bordered nerve fibres, while the ramifications of another, the pointed process, terminate *directly* in the terminal fibrillous network, the meshes of which are filled up by small elementary granules. Directly from this network, as I have shown above, another set of very fine nerve fibres arises, which also pursue their course toward the periphery. In the cortex of the cerebellum we meet the same arrangement; the ramifications of the enormous processes of the ganglionic bodies of *Purkinje,* namely, terminate in the network of the grey layer, while their small *basal* processes are transformed into dark-bordered nerve fibres, which finally form a part of the white substance. From the terminal network of the grey layer, the same as in the cerebrum, a considerable number of fine fibrillæ arise, which subsequently form the axis cylinders of dark-bordered nerve fibres, a part of which pass around the sulcus to a neighbouring convolution, in order to form a communication between the nervous elements of two adjoining convolutions, while the rest proceed to the white substance.

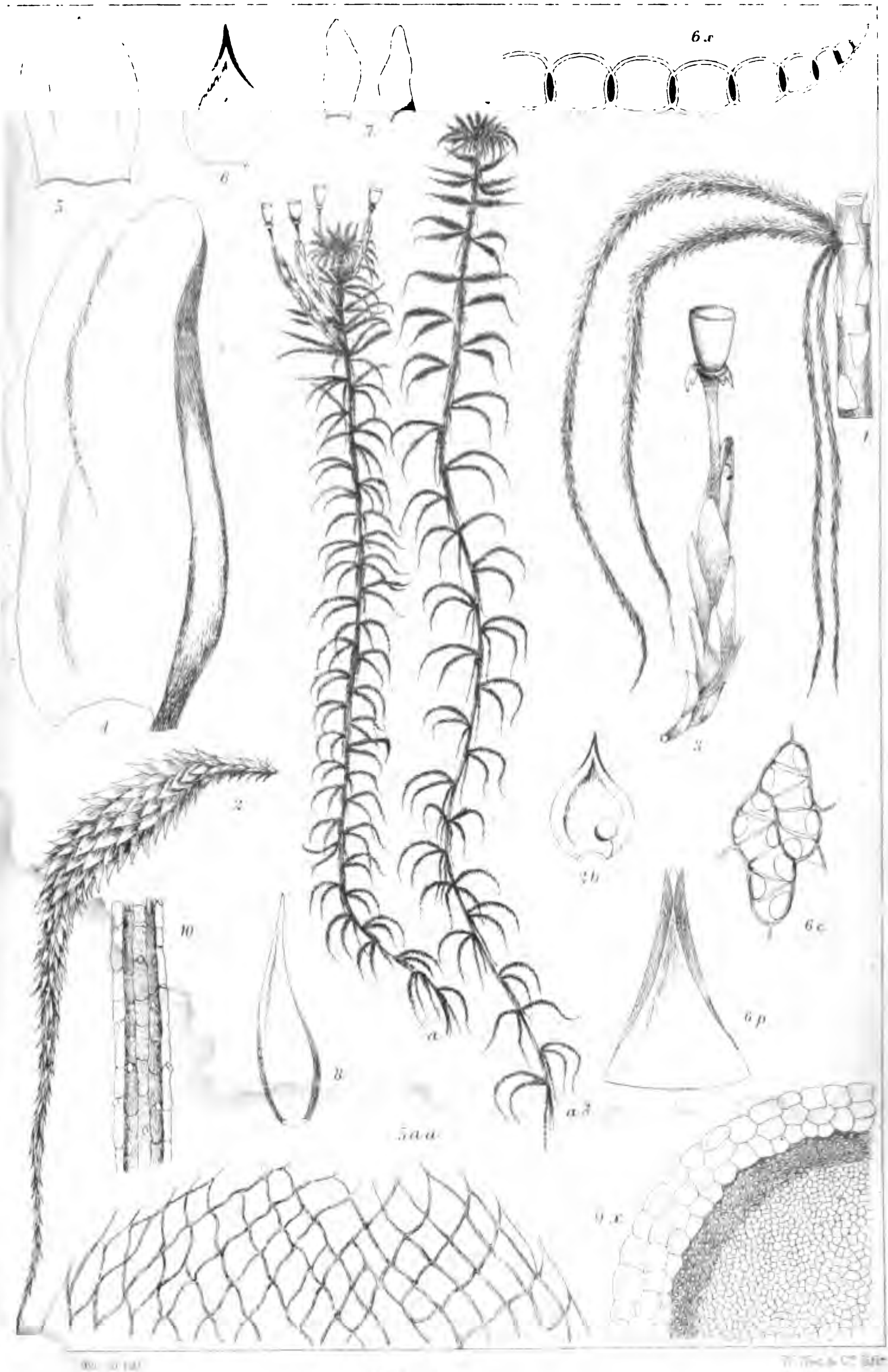
In the sympathetic ganglionic bodies, finally, we may compare the *body*, enclosed within the capsule, to a ganglionic body of the cortical layer of the cerebrum, while the *capsule* itself—representing a fine fibrillous network, the meshes of which are filled with small granules—is *analogous* to the terminal network in the granular substance. From the body, enclosing its nucleus as all other ganglionic bodies, we observe some processes arise, which, after having pierced the capsule, give rise to axis cylinders which are finally transformed into dark-bordered nerve fibres, while the ramifications of a number of smaller processes, also arising from the body, terminate in the network of the capsule. From the outer surface of the latter, lastly, a number of fine fibrillæ arise, a part of which establish a communication with the adjoining capsules, while the rest unite to form the ultimate sympathetic nerve fibres, going to the periphery.

5. *The Nerve Fibres and Ganglionic Bodies of Insects.*—The *nerve fibres* of insects consist of fine *granular* fibrillæ, about $\frac{1}{1200}$ mm. in diameter, enclosed within a structureless sheath, and thus representing the entire nerve. In the nerve fibre of an insect, therefore, we behold the conducting anatomical element of the nervous tissues in its primitive form, that is, as a simple axis-cylinder fibre. The history of the development of these tissues in the human embryo justifies this view. The fibrillæ running parallel to each other in the sheath of the nerve are not divided into subordinate bundles. They are surrounded by a semi-liquid substance, resembling in character the nerve-medulla of the dark-bordered nerve fibre of the higher animals.

The *ganglionic bodies* of insects are round or slightly oval bodies, from each of which a large process, composed of fine fibrillæ, arises. They consist of a mass of granules, surrounding a large, clear nucleus. The latter, about $\frac{2}{75}$ mm. in diameter, shows a fine double contour, and contains a greenish shining nucleolus of $\frac{1}{200}$ mm., composed of several granules. From the surface of the granular mass surrounding the nucleus a great number of fine *short* fibrillæ arise, which slightly anastomose with each other. From these anastomoses others arise, the greater portion of which join in forming the large process, while the rest pass over to the adjoining ganglionic bodies to establish a reciprocal communication. A sheath, enclosing the whole body, seems not to exist. The ganglionic bodies of a ganglion are, like those of the sympathetic nervous system of the higher animals, divided into groups. The processes arising from the bodies of a group, unite to form nerve-fibre bundles, and these, again, unite with those of other groups, forming still larger bundles, from which, finally, the larger nervous trunks, leaving the ganglion, arise. The whole ganglion itself is sur-

rounded by a structureless membrane, which extends, in the form of the above-mentioned sheath, over the nerves. The interspaces of the ganglionic bodies, and the bundles of nerve fibres within the ganglion, are filled up by a semi-fluid substance containing innumerable granules.

[*Errata.*—In my paper on the development of the blood corpuscles, in No. LXII., February, 1874, p. 49, line 10 from top, for "blood," read "brood"; and 18 lines from top, for "blood," read "brood." Also the words "nat. size" on Plate LI. is, of course, an error.]


6 x
5
6
7
1
4
3
2
2 b
6 c
10
6 p
8
a
a 3
5 a a
9 x

II.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Plates LXVII. and LXVIII.

14. Sphagnum squarrosum Persoon in lit.

Weber and Mohr, Reise durch Schweden, p. 29, Tab. II, fig. I *a. b* (1804).

Plate LXVII.

Syn.—Smith Eng. Bot. t. 1498 (1804).—P. de Beauvois Prodromus p. 88 (1805).—La Marck et Cand. Fl. Franc. I, p. 443 (1805).—Schultz Fl. Stargard. p. 276 (1806).—Bridel Sp. Musc. I, p. 14 (1806). Mantissa p. 2 (1819). Bry. Univ. I, p. 5 (1826).—Web. & Mohr Bot. Tasch. p. 73 (1807).—Schkuhr Deutsch. Moose p. 14, t. 6 (1810).—Schwägr Supp. I, P. I, p. 13, tab. IV (1811).—Röhling Ann. Wetter. Gesells. I, p. 197 (1809). Deutsch. Fl. III, p. 36 (1813).—Voit Musc. Herbip. p. 12 (1812).—Hook. & Tayl. Musc. Brit. p. 4, tab. IV (1818).—Funck Taschenh. t. 2 (1821). Zenk. & Dietr. Musc. Thuring. Fasc. I, No. 21 (1821).—Nees. Hsch. & St. Bryol. Germ. I, p. 9, t. 1, fig. 3 (1823).—Hübener Musc. Germ. p. 23 (1833).—De Notaris Syll. Musc. Ital. p. 295 (1838).—C. Müll. Synop. I, p. 94 (1849).—Wilson Bry. Brit. p. 23, tab. IV (1855).—Hartm. Skand. Fl. ed. 6, p. 435 (1854).—Schimper Torfm. p. 63, tab. XXII (1858).—Synop. p. 677 (1860).—Lindberg Torfm. No. 7 (1862).—Berkel. Handb. Br. Mosses p. 308, Pl. 2, fig. 4 (1863).—Russow Torfm. p. 62 (1865).—Milde Bry. Siles. p. 387 (1869).—Hobkirk Synop. Br. Mosses p. 26 (1873). *Sph. Aconiense* De Notaris MSS. *Sph. patulum* Mitten MSS.

Monoicous, in loose deep glaucous green tufts. Stems robust, 6–15 inches high, generally dichotomous, rigid, reddish brown. *Cortical cells small, non-porose, in two strata,* the woody zone rufous brown. *Cauline leaves* large, erect or reflexed, *lingulate, not bordered,* minutely auricled at base, *apex rounded, slightly fimbriate;* hyaline cells elongated hexagono-rhomboid below,

---

EXPLANATION OF PLATES.

Plate LXVII.

Sphagnum squarrosum.

a.—Fertile plant.
1.—Part of stem with a branch fascicle and male inflorescence.
3.—Perichætium and fruit. 4.—Upper bract from same.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same.
6.—Leaves from a divergent branch. 6 *x.*—Section. 6 *p.*—Point of same. 6 *c.*—Cells from middle, × 200.
7.—Basal intermediate leaf. 9 *x.*—Part of section of stem. 10.—Part of a branch denuded of leaves.

Plate LXVIII.

Sphagnum teres.

a.—Female plant. *a b.*—Male plant.
1.—Part of stem and a branch fascicle.
2.—Male inflorescence. 2 *b.*—Bract from same with antheridium.
3.—Perichætium and fruit. 4.—Upper bract from same.
5.—Stem leaf. 5 *a a.*—Areolation of apex of same.
6.—Leaves from a divergent branch. 6 *x.*—Section. 6 *p.*—Point of same. 6 *c.*—Cells from middle, × 200.
7.—Basal intermediate leaf. 8.—Leaf from a pendent branch.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

rhombic above, without fibres or pores, but here and there with a transverse partition.

Ramuli 4–5 *in a fascicle, of which* 2–3 *are divergent, tumid attenuated toward the points, with the leaves on the lower two-thirds squarrose and recurved from the middle,* those of the upper third imbricated and elongated; the other branches pendulous and appressed, slender, terete, with all the leaves imbricated; cortical cells elongated, in two strata, the retort-cells perforated but scarcely prominent at apex. *Branch leaves from a very concave base, broadly ovate, suddenly becoming lanceolate above, the margin involute in the upper third, the apex minutely* 3–4 *toothed, bordered by* 2–3 *rows of very narrow cells;* hyaline cells with numerous annular fibres and two rows of large pores, *chlorophyll cells compressed entirely enclosed by the hyaline.*

Male amentula terete, clavate, yellowish-green; the bracts slightly squarrose, oblong-lanceolate, the basal cells without fibres and pores, the upper shorter, with annular fibres and small pores. Fruit seated in the coma or in the axils of the upper fascicles, moderately elevated; *the bracts somewhat distant, concave convolute,* the lower oblongo-elliptic, *the upper very broad, obovate emarginate and slightly fimbriate at apex,* laxly areolate, without fibres or pores. Spores yellow.

Var. *β, squarrosulum. Sph. squarrosulum* Lesquereux.

Plants smaller, more slender, deep green above, pale below. Stem pale green. Leaves small, more distant.

Hab.—About boggy springs and the sides of moorland streams. *β*, in more shady alpine places. Fruits in July. Not uncommon, and found throughout Europe and the middle and northern states of North America. This fine species sometimes attains a great size, becoming proportionately robust, and thus may be looked upon as the grandest European representative of the genus. It may be readily recognized by its squarrose leaves, and often bears fruit abundantly. Lindberg considers *squarrosulum* Lesq. to be rather a starved or undeveloped form than a distinct variety, yet it is widely distributed, but does not appear to have been ever found with fruit.

15. Sphagnum teres Ångstrom.

Hartm. Skand. Fl. ed. 8, p. 417 (1861).

PLATE LXVIII.

Syn.—LINDB. Torfm. No. 6 (1862).—MILDE Bryol. Siles. p. 388 (1869). *Sph. porosum* LINDB. MSS.

Sph. squarrosum var. γ teres SCHPR. Torfm. p. 64 (1858). Synop. p. 677 (1860). —RUSSOW Torfm. p. 64 (1865).

Dioicous, in small tufts or intermixed with other species, soft, pale yellowish green often with a ferruginous tint. Stems slender,

4–8 *inches high, pale rufous red; the cortical cells non-porose, in three strata, the woody zone red.* Cauline leaves precisely like those of *S. squarrosum*. *Ramuli distant*, 4–5 *in a fascicle*, 2–3 *divergent, terete; the leaves imbricated throughout, and only having the apices slightly recurved; broadly ovate, pointed, three toothed*, in structure agreeing with those of *S. squarrosum*. Cortical cells of branches in a single stratum.

Male amentula elongated, brownish, fertile and thickened in the lower part, and beyond this extended into a slender sterile branch; the bracts broadly ovato-lanceolate, pointed, agreeing in structure with the branch leaves.

Fruit seated in the coma, or in the upper fascicles; the bracts resembling those of *S. squarrosum*.

Hab.—About the edges of bogs and springs in subalpine districts; sparingly distributed. In this country it has been found by Mr. Wilson at Knutsford Moor, Wybunbury Bog and Newchurch Bog in Cheshire; by McKinlay at Doune; and by Mr. Stabler at Staveley, Westmoreland.

This plant has usually been regarded as a variety of *S. squarrosum*, and Professor Lindberg has recently expressed to me his coincidence with this view; structurally there is absolutely no distinction between them, but in external aspect *S. teres* closely resembles *S. strictum*. The beautiful and instructive specimens collected by Limpricht at Bunzlau, and distributed under No. 1153 of Rabenhorst's Bryotheca, combine the characters of both, the upper part having the imbricated leaves of *S. teres*, the lower part the squarrose leaves of typical *S. squarrosum*. There is thus left to us only the dioicous position of the inflorescence, and the slight difference in the male amentula.

III.—*The Optical Quality of Mr. Tolles' ⅛ Objective.*

By Robert B. Tolles.

Further but brief discussion of the well-whipped subject of angular aperture is here offered on my part, being again summoned to the front after repeated dismissals by Mr. Wenham, while he imparts "further information" as to optical law concerned. I

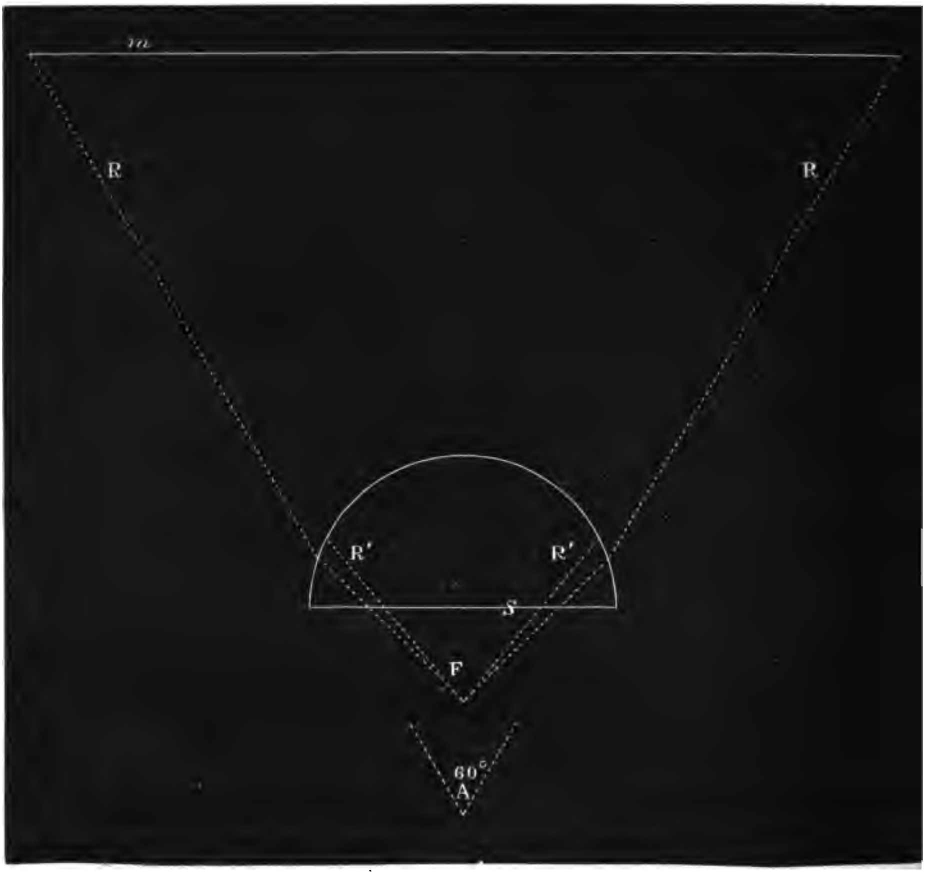
EXPLANATION OF FIGURES.

Fig. 1.—m, Front surface of middle.
R, R, Outside rays of pencil from middle 60°.
R′ R′, Pencil traced *from Focus* of 82°.
Outside pencil *at Focus* of 90°.
„ 2.—Angle reduced by *closing* the lenses.

allude to his notice of the $\frac{1}{8}$-inch objective of my manufacture, and owned by Mr. F. Crisp, of London.

Without quoting his declarations and conclusions about that objective, I refer the reader to the accompanying diagram, Fig. 1, as an illustration of the general case. I offer it as an example of the conditions under which balsam-angle can be obtained greater than

FIG. 1.



can possibly proceed from an air-angle capacity of the maximum extent. That corresponding maximum balsam-angle is, as all constantly agree, 82° very nearly. This is elementary and plain—assuming, however, that the material of front or of front-surface is 1·525 refractive index.

But to discuss the *other* case, *i.e.* of balsam-angle exceeding 82°.

Let the lines marked R, R in Fig. 1 represent the extreme marginal rays of a pencil of 60° angle proceeding from the inner systems, *viz.* the back and middle combinations acting as one. Angular aperture of *such* limited extent, 60°, certainly involves no difficulty as to measurement, and hoping no objection from any critic on that score, I will assume the angle and focus-distance A of the pencil emergent from the front surface of the middle, *m*, to be accepted on trust. In support of the case, I will state here that I can easily give *more* available angle to back-and-middle.

When the front, which is a hemisphere, is in position, as shown in Fig. 1, the cone of light bounded by R, R is intercepted by it at its convex surface, and brought to a focus at F *directly*, the front being of common-crown glass, and "immersed" in balsam of the same refractive-index, assumed to be 1·525 for the case. The focal-point F is obtained by projection, and Mr. Wenham is expected to *admit its correctness* or show the contrary. The angle at F is 90°.

And now to discuss the case of maximum air-angle under the same circumstances.

The inner lines R′ R′, Fig. 1, are drawn at an angle of 82°, or 41° with the axis. As this pencil of smaller angle emanates from the same focal-point F *in balsam*, the rays R′ R′ must meet the convex surface of the front at a less distance from the axis than the larger pencil, as may be seen in the figure. But as this reduced aperture of the lens at the front, and at the convex surfaces, is all that can possibly be used "dry," its limits being shown by tracing the course of extreme rays for the equivalent balsam pencil of the largest possible pencil transmissible to or from air; it follows that that portion of the front-surface between the lines R′ R must be unused aperture, when the objective is used dry, though the practical angle be infinitely near to 180°.

To illustrate a little further: Let the light be considered as proceeding down the tube from the eye-piece, and the objective being *dry* at the surface, *s*. Obviously the extreme ray of the pencil of 82°, and of balsam-focus at F, would meet the interior plane and dry surface of the front at an incidence of 41°, and necessarily have its course on emergence with a *nearness to coincidence* with the front plane surface, involving only an inappreciable difference from a direction parallel, or 180° of angle.

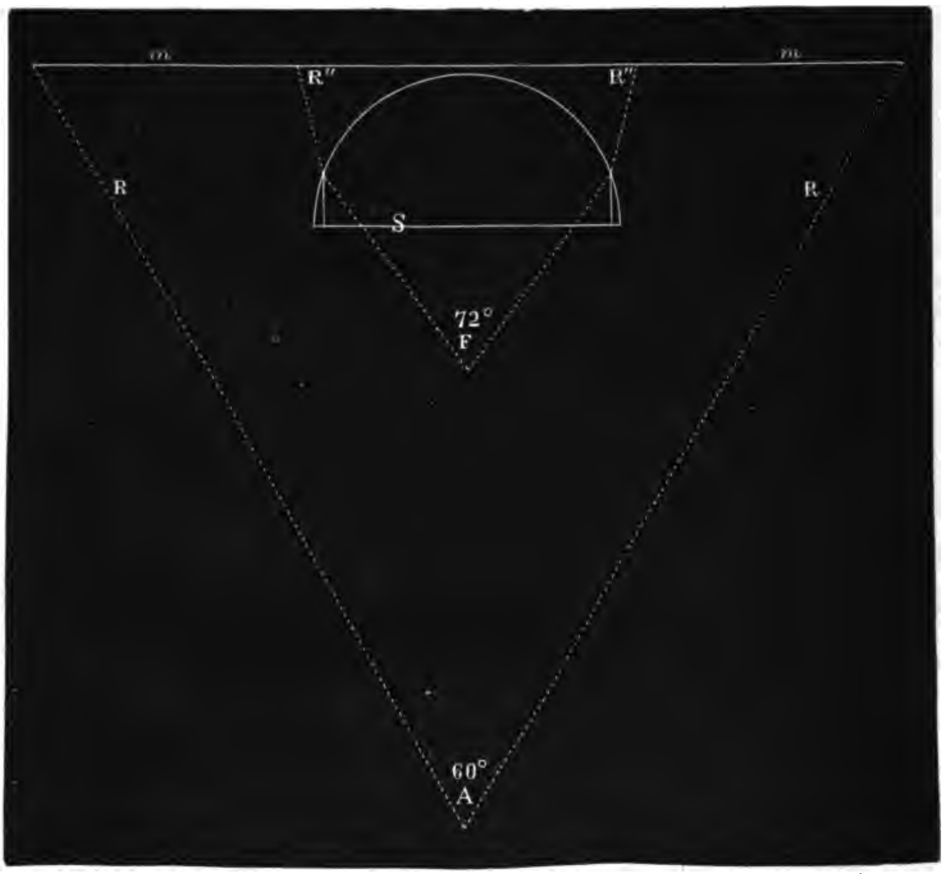
Mr. Wenham's small caps may perhaps properly enough apply here to express *his sensations*, but the *fact* becomes clear and indisputable.

Dry, the surplus aperture from R′ to R, *i.e.* beyond 41° of incidence, gives a silvery ring by total reflexion from the plane surface as should be expected. It trims the aperture like a diaphragm.

More Balsam-Angle.

By increasing the angle of the back-and-middle pencil of course more balsam-angle of the objective would be obtained, at the same time using more clear opening of the (this) front. But the limit of angle of the inner systems being attained, not that gains for us the limit of possible balsam-angle, though considerably beyond 90° could be gained thus. The front can be made to play a part it does not here; helping to a still further increase.

Fig. 2.



Place of Adjustment for Maximum Angle.

A case of reduction of balsam-angle by mere change of position to closed is shown in Fig. 2. The same front is represented in

near-contact approach to middle. The same back-and-middle and angular pencil are concerned, and nothing changed except relative position effected by closing the adjustment. The *clear opening* of the front being the same as before for 90°, its boundary is reached by the pencil indicated by the lines R″ R″ proceeding from the middle. Projecting the course of these rays their focus is found at F, and shows a reduction of angle from 90° to 72°, or slight fraction more. Although *closed* NOT the maximum balsam-angle.

Spherical Aberration.

Having shown that the *closed* point is not necessarily the adjustment for maximum angle, I wish to call attention to the enormous aberration under which angle is taken ordinarily, without "cover," and, I will assume, the systems closed.

With a dry front surface the same extreme rays R″ R″, Fig. 2, will have, when proceeding from within to air, refraction to the *corresponding* air-angle; but not any ray *nearer to the axis* than those, can have the same focus in air.

Take the case of light radiating from a small point in the axis of the microscope at the place of the eye-piece, or, which is the same, the longer conjugate focus. The focal point at the front surface will move to a point of gradually *increased* distance, as the rays from the 10 inches distant radiant point are incident at the convex-surface of the front at a gradually *decreased* angular distance from the axis.

This enormous spherical aberration arises at the front plane surface, and for compensation, or correction rather, needs a covering glass of suitable thickness; with a technically "dry" objective this would be sufficient. But with an immersion objective having immersion contact with "cover" to a dry-mount, the dry front surface of the objective is in effect removed to the surface of the covering glass *next the object;* and the focus of the *extreme* marginal rays will then be very near to the focus of the *mean* and nearly central rays, *because* all the rays of the cone emerge within a comparatively *very small area* about the axis. This is all very well known and, though the demonstration is easy, that may be omitted.

I do not care to comment on Mr. Wenham's precautions in taking angles, as described in his account of the $\frac{1}{8}$-inch. I will only say that the whole seems to me quite unnecessary.* What I have said of interposition of "cover" is suggestive of the true

* Mr. Wenham, in his diagram at p. 114, the lower one, draws the ray *d* for 170° of aperture at an impossible emergence from the convex surface. It falls outside of the perpendicular and must be bent *outward*, the surface acting as a concave. Perhaps Mr. Wenham thinks this of no consequence and—so do I!

method I judge, and it is this—for a *strict test* of angle let the objective be measured in *position as used* on the object.

Mr. Wenham having adopted the semi-cylinder, I will remark that this can be very easily done with that, in all cases; but as an effectual means to dismiss any suspicion of false light, "not going to form images," let this, which he has well approved, be done, *viz. put light down through the tube of the microscope.* Then if a low eye-piece be put in place in the body, and a beam of *sunlight* be the light used and directed through the eye-piece down the body along the axis, it will have emergence at the cylindrical surface, and, if intercepted by a band of thin moist paper adhering to the same surface, the *circular boundary* of the emergent pencil will be sharply defined, and the angle can be accurately and instantly read there.

Of course, if a *dry mount* is used, only 82° can appear emergent at the surface when the air-angle of the objective is infinitely near to 180°, and reference index of cylindrical piece being 1·525, as Mr. Wenham used to stipulate *for balsam*, with a view, it is understood, to simplify the argument, its index varying slightly from that.

Whatever the angle read at the cylindrical surface, the corresponding air-angle is readily known or deduced from it; for surely it will not be claimed that other than "image-forming rays" will have emergence from the objective-front when derived from a beam of unmodified sunlight entering the eye-piece.

And now, though claiming for that method that it is safe against false light, yet I admit (or claim) that sunlight through the eye-piece and thence through the objective is not necessary for accurate results; not at all; for I have always found the results of the *reverse* method to nicely correspond—*without* any diaphragming of the field, such as Mr. Wenham describes in the 'Monthly' for March last, it will be found that the limits of angle are the *same,* whether the light be put down through the body, or reversely, the light has direction from a radiant in front, and angle-limits observed at the eye-piece.

There is especial convenience, however, in the latter method, and having happily Mr. Wenham's endorsement of the semi-cylinder for such purpose, I will particularize. Thus if the cylindrical piece, balsam-mounted-object thereon, and objective be in position, and the observer having the object in plain view through the microscope, then, with lamp in hand, while moving the screw-collar forward or back for the limit of angle as the radiant is moved out from the axis, the focal adjustment being maintained consentaneously, the real maximum angle will be *ascertained* by working for the *greatest* obliquity of the illuminating ray that bisects the field. When that greatest obliquity of incidence is gained, *measuring* the angle can be attended to.

My test measurements of angle were made according to this method. An exact semi-cylinder in use, carefully in proper position, and an object mounted in balsam under covering glass immediately on the plane surface of the semi-cylinder itself. It was used where a balsam-angle of 110° was shown, the method described, and with the result published.* The method has not been, and will not be, impeached. The results were *verified* by putting the light (sunlight) down through the tube.

When the maximum angle limits have been *ascertained*, then the angle can be *measured* in the way Mr. Wenham points out, *viz.* by rotating the microscope. Or, if the semi-cylinder be provided with divisions to degrees, adjacent, a shutter sliding over the cylindrical surface from the central parts *outward* will, under observation at the eye-piece, mark exactly the limits of angle on either side, and the corresponding degree can be noted. According to Mr. Wenham, only 82° can be seen there with an object-glass properly so called, with air or water or balsam contact at plane surfaces. Now, note the fact that in my hands in June last † this occurred—*viz.* when 81° closely was shown on the *screen* as the aperture of an objective "dry," then, by simply making the medium of contact at the plane surfaces water, or balsam, instead of air, the pencil-limits on the screen were found at about 110°, the object *in focus* in both instances.

And then, to show that this additional pencil consisted of "image-forming rays," the moist-paper screen was removed, and a shutter, covering 82°, substituted, and on illuminating the object through this outside portion only, behold, with the objective at maximum angle adjustment and *used dry*, total darkness was the effect of course to the eye at the eye-piece, while if balsam or water contact was given, the light had access beyond 82°, and the object became in clear view well defined.

Thus fact and "theory" continue (!) consistent.

JACKSONVILLE, FLORIDA, *May* 14, 1874.

IV.—*On the Structure of Diatoms.* By G. W. MOREHOUSE, U.S.A.

IT is hoped that the publication of the following memoranda will serve the double purpose of elucidating the structure of the tests, and at the same time demonstrating the utility of microscopical objectives of exceptionally high powers. The uncertainty of the footing in this unstable and contested ground will necessitate many errors, and may serve as an excuse for them. So many competent

* See 'M. M. J.' for June and August, 1873.

† See 'M. M. J.' for August, 1873.

microscopists have written upon this subject, that the writer would fain be silent were it not for a firm belief in the superiority of the instrument he used, for this kind of investigation. In fact this excellent glass gives advanced work on almost every test tried, and fully justifies the confidence reposed in it. The observations recorded below, unless where otherwise stated, were made with a Tolles' $\frac{1}{50}$ immersion objective of 165° angle of aperture, and generally a Tolles' 2-inch eye-piece, giving an amplification of 2500 diameters.

Eupodiscus Argus.—My attention was especially called to this shell by having noticed the wide difference between the views of Mr. Henry J. Slack, Mr. Samuel Wells, and Mr. Charles Stodder. My observations are corroborative of the idea of two plates, as asserted by Messrs. Stodder and Wells. Using a $\frac{2}{3}$ objective with power of 340 times obtained by high eye-piece and extending draw-tube, and using a Lieberkühn, the outside or coarser markings on specimens mounted convex side uppermost are white with white cloud illumination. An erased space on one shell and the holes or depressions through which Slack's four large "spherules" are seen, are now black. They, then, are not covered by the external "crust."

The slide was then turned over and the inside of the same specimen examined by the same method, and on the more favourable portions of it the finer network of the *inner plate* is also seen in white, the "spherules" being perfectly black. By this reflected light the "four spherules" are plainly seen to be dark openings in the white plate, and the network is clearly traced across the areolæ in the outside plate. The diatom looks like a piece of coarse white netting laid over a finer piece.

Under the Tolles' $\frac{1}{50}$ and with transmitted light, whether central or oblique, it matters not, all portions of the surface of both the upper and the lower plates are found to be covered with, or composed of, a *still finer network* with irregular oval meshes like the two coarser ones.

The place in the shell above referred to, where the layers are erased, denuding an interior structureless "vail," gives an opportunity to observe the edges of the fractured layers. The broken edges of both plates bordering on the erasure show the jagging of this finest net structure. The arrangement of the finest areolæ is more regular near the margin of the diatom, or appears so by reason of the simpler character of the structure in that part. They are easily seen with the $\frac{1}{50}$ on any part of every specimen studied, but are unusually distinct between the "four spherules" on the inner plate looking through the largest openings in the outer plate; or may be rendered still more distinct on shells with the concave side up. They are more difficult to be seen on the outside crust with

the high powers used, because of its greater opacity. Deductions from focal changes with reference to the various markings lying in different focal planes corroborate the conclusions above expressed.

The disks examined are on Möller's Probe-Platte, and on a slide prepared by Mr. Wells.

Hyalodiscus subtilis Bailey.—On this beautiful little shell the "engine rulings" are readily seen with almost any illumination, and the inevitable concomitant of intersecting lines, whether real or illusory beading is displayed. When we use monochromatic light the whole scene is changed. The hyaline portion of the disk is instantly resolved into perfectly well defined hexagons, radiating from the central nucleus. The central part, because of its greater depth and complexity, is only resolved into irregularly shaped spaces of a more or less hexagonal form. Every one of the five beads usually seen represents the centre of an hexagonal plane exactly as in *Pleurosigma angulatum.*

The hexagons are well defined with a power of 7000 diameters. They may also be seen with lamp or daylight.

Triceratium favus.—The two sets of markings on this fascinating object certainly lie in different focal planes,* and probably "belong to two distinct layers." The coarse hexagonal ridges are found to project from the outer or convex surface, and the inner plate bears the minute markings. This is proved by the fact that the fine markings show decidedly the plainest on valves that are mounted with the interior surface uppermost.

Under this superior objective the finer markings, like the larger, are distinctly faveolate. Their hexagonal structure is easily seen even with lamp illumination. When examining comparatively thick shells, possessing a complex structure, like the one in question, the necessity for avoiding errors caused by too intense or by excessively oblique light becomes at once apparent. The unequal refraction of the light in passing through the external silicious layer produces a distorted image upon and of the interior surface. In this manner distorted small hexagons may be seen along the lines of the larger network by a lens incapable of clearly displaying the minute hexagonal markings above described.

The best results are obtained on the *T. favus* with a moderate light nearly central.

Surirella gemma.—This beautiful form has been subjected to all the different conditions of illumination in my possession. Like other relatively thick shells, the appearances presented by the markings vary greatly with the changing conditions of observation. No trouble is experienced in bringing out the longitudinal striæ, nor in making the little beauty *seem* to "wear beads." At times the beads give place to rectangles, and again after careful manipulation to

* See Carpenter, 'The Microscope,' 4th edition, p. 282 and note.

sharply-defined elongated hexagons.* Hartnack's hexagons as figured are too much elongated; although sometimes such an appearance is presented when the illuminating pencil is at right angles with the median line, the transverse lines being less distinctly perceptible. When the light is so arranged as to show every side with equal perfection, the form of the markings is nearer that of regular hexagons.

The Amici prism is found to work excellently on the *Surirella*, and when it is used with the $\frac{1}{50}$ objective and the blue cell the slightly elongated hexagons are easily exhibited on an average frustule.

Aulacodiscus Kittonii.—This splendid disk is traced with easy angular figures evidently elevations, and the spaces between the lines are undoubtedly depressions. Some of the markings are circular, others square, some pentagonal, some hexagonal, and others heptagonal. Broken specimens of *Brightwellia Johnsonii* with like surface markings show the line of fracture running through the areolæ.

Navicula rhomboides.—Individual frustules of this species vary considerably in degree of difficulty of resolution. Some of the smallest valves when mounted in balsam tax the powers of excellent instruments. The writer has found all specimens, whether mounted dry or in balsam, to yield readily transverse striæ with oblique illumination direct from the lamp. Under the same conditions an average valve exhibits well-defined longitudinal striæ. With the ammoni-sulphate cell it is instantly and clearly shown covered in every part with squares, like *Pleurosigma Balticum.*

Navicula crassinervis.—The specimens of this variety, in my possession, are more difficult than *Frustulia Saxonica*, and even rival *A. pellucida* under lamp illumination; but any clean frustule is satisfactorily resolved.

Using monochromatic light with plain mirror and Wenham's paraboloid, *longitudinal* lines are discovered. After careful manipulation both sets of lines are seen at the same time, and an appearance of beading results.

Navicula cuspidata.—Both sets of lines are easy, but the longitudinal are much closer together than the transverse. Consequently the light interlinear spaces are elongated and no semblance of beading is to be seen. In diatoms where the intersecting striæ are of nearly equal fineness the little square spaces, when not well defined, seem circular; and if the illumination by transmitted light is intense, they present a raised appearance due to refraction.

Mr. Charles Stodder called my attention to this diatom with the view of ascertaining with the $\frac{1}{50}$ whether or not the two sets of lines lie in different focal planes. My observations, many times

* 'The Microscope,' Carpenter, page 182.

repeated, have convinced me that they are never both in focus at the same time, and further that the longitudinal lines are on the external surface, and the transverse on the internal plate. If there are not two plates, the lines may be on opposite surfaces of the same plate.

White cloud illumination is found to be much better than other and more brilliant light for demonstrating these slight differences in focal distances. Many errors of interpretation are avoided by using an approximately central pencil when the instrument used is capable of elucidating all the details of structure without greater obliquity.

Frustulia Saxonica.—In addition to my observation of longitudinal lines upon this test and resolution into dots,* it may be worth noting that even with lamp illumination the $\frac{1}{50}$ has displayed the transverse much clearer than they appear in Dr. Woodward's photo-print.† With oblique light direct from a small German student's lamp, without mirror, prism, or condenser of any kind, a person entirely unaccustomed to the microscope could distinctly see them with Beck No. 3 eye-piece, power 7000 times. With the ammoni-sulphate of copper cell the longitudinal lines and dots are displayed with ease.

This is one of the most difficult test diatoms thus far studied, ranking but little easier than *A. pellucida*, *N. crassinervis*, and *Nitzschia curvula*.

Amphipleura pellucida.—Many times the writer has been able to confirm the observations of longitudinal lines on this most difficult test shell, but never has succeeded in seeing the dots except with the blue cell and Wenham's paraboloid, and only then under favourable circumstances.‡ When resolution is effected, the dots are exceedingly minute and uniform in size, showing as mere points of light when magnified 2500 times. On one occasion the writer has seen fine dark lines crossing between the transverse striæ like the steps of a ladder, the dots or spaces plainly longest in direction parallel with the median line, proving the longitudinal to be finer than the transverse lines.

One obstacle in the way of resolution of the longitudinal striæ is the presence of diffraction lines. The valves being so narrow increases this difficulty. Only after much time is wasted, and after many discouraging failures, will the patient observer receive the reward of success.

Nitzschia curvula Sm.—The unusual number of spurious appearances in this object leads me to suspect that it possesses a

* 'American Naturalist,' July, 1873, p. 443.

† 'Lens,' vol. i., p. 197.

‡ See I. E. Smith, in the 'Lens,' April, 1873, p. 115. See also the 'American Naturalist,' May, 1873, p. 316.

complicated structure, as yet beyond the reach of the instrument. The extreme fineness of the longitudinal lines, as compared with the transverse, reminds one of the *Navicula cuspidata*, and as is the case with the coarser shell, no efforts avail to develop a semblance of beading.

Striatella unipunctata.—Two sets of fine lines, and as the direction of the light is changed, may be made to exhibit either beads or squares. In point of value as a test, will be found to approach *Surirella gemma.*

Grammatophora.—Of this genus the writer has examined the *G. marina*, *G. subtilissima*, and *G. serpentina;* all of which are resolved into hexagons. Broken specimens of *G. marina* show the line of fracture running through the hexagonal planes and leaving points of the network projecting. The markings continue completely illustrated as the stage is revolved, in whatever direction the beam of light may fall.

Stauroneis.—Some of the larger varieties of *S. phœnicenteron* are covered with hexagonal areolæ, easily exhibited with central daylight. The projecting points of the fractured partitions between the hexagons may be observed.

Pleurosigma angulatum.—Hexagons. The line of fracture generally running around them, but quite often through them.

Pleurosigma Balticum.—A drop of water slowly advancing by capillary attraction shows this shell to be covered with squares, and proves that both sets of lines forming the boundaries of the squares are on the same surface of the valve; and the appearance presented by an air bubble on the other side proves that surface to be smooth.

Pleurosigma formosum.—Near the ends of the frustule it is easy under certain adjustments of the light to make it appear like a checker-board with alternate bright red and green squares. Double rows of green and red beads alternating may be seen on this as well as on other species of the same genus.* When we resort to central light from a white cloud, and thus lessen the liabilities to err caused by refraction, diffraction, decomposition of light, and oblique projection of shadows, the conclusion is arrived at that these various appearances are caused by two sets of intersecting diagonal ridges, the finer ridges running up and down, over and between the coarser, and subject to considerable variation even on the same frustule. This theory would also account for the "beads" (?) being of different colours, and the same "beads" changing colour when the focus is changed. We see in many of the mollusks shell-markings of a similar character.

Concluding remarks.—It would seem that the perfect box-like form of the shells of the Diatomaceæ and their elaborate orna-

* Dr. Pigott, in 'M. M. J.'

mentation would exclude the idea of a blind process of chemical crystallization. Analogy should teach that they are secreted for a protective covering for the tender animal-like plant, as among higher forms. If this is true, the surface markings ought to be so distributed as to give additional strength to the shell without greatly adding to its weight. It would also be expected that some of the larger shells would be perforated with holes. This idea, of course, would have to admit into the discussion considerations of habits of growth, and environments. Those contained in gelatinous envelopes should be less developed in strength of shell and bracing. Those growing on algæ, and in exposed localities, should be strong to resist fracture. On those moving free the bracing would be in proportion to the weakness of the shell; larger shells being relatively more liable to be broken. Here as elsewhere nature, without waste of material, combines utility with beauty.—*The American Naturalist*, May.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Muscular Tissue of Corethra plumicornis.—This is very fully described in an article in a late number of Max Schultze's 'Archiv,'* by Herr G. R. Wagener. He enters upon the subject of its development, and he points out many facts which show that it is in its development not unlike the ordinary striated muscle of vertebrate animals. A couple of excellent plates, done in the 'Archiv's' best style, accompany the paper.

The Development of the Ovum in Anodonta.—This is a subject which is partly worked out by Herr W. Flemming, who is Professor of Anatomy in Prague. He shows among other things the peculiar form of the zoosperms and the mode in which they enter through the micropyle. This paper, which extends over more than thirty-five pages, is illustrated by a good plate.†

Has the so-called Microsporon Audounii any Existence? — This question is very well discussed in the 'Archives de Physiologie' (May, 1874), by M. Malassez, who, having debated the question very fully, arrives at the conclusion that it is present, but differs to some extent from the conclusions already laid down by M. Gruby. M. Malassez says that it is constituted of minute spores, of which he describes three types:—(1) Those which measure from 4 to 5 mm. have a double contour and may possess buds: these are the large spores. (2) Those measuring from 2 to 2·5 mm. have no double contour, and may have buds: these are the small spores. (3) These have an inferior diameter about 2 mm., have a single contour and no buds: these are the sporules. He says that the ovoid spores that one sometimes sees do not belong to this fungus, but most probably to some other one. There are no tubes, but sometimes little chains of five or six or even more spores. These results differ from those of M. Gruby chiefly in the absence which they record of branches and stems, which are described by M. Gruby to be present.

The Zoological Position of the Hypopus is very clearly defined in a memoir by M. Megnin in the 'Journal de l'Anatomie' (No. 3, May, 1874). These animals belong to the acari class of beings, and are parasitic (so-called) on nearly every animal, from the common house-fly upward. M. Megnin discusses their relations to each other, their anatomy and their habits, and comes to some interesting conclusions; among others, that these animals are not truly parasitic; for he says that the animal on which they are situated merely acts as a "*disseminator* and preserver of their species." This paper is accompanied by four good plates, in which several species are figured in their mature and immature conditions.

The Structure of the Skin.—Dr. P. H. Pye-Smith gives an excellent survey of the recent work in this direction, which has been done by

* 1874, Zehnter Band Drittes Heft.
† See Max Schultze's 'Archiv,' 10th Band, 3rd Heft.

Professor Tomsa, of Kiev, in Russia. Dr. Pye-Smith says ('Medical Record') that the author, starting from the zones recently sketched by Langers, describes the felted arrangement of the fibrillæ in the papillary layer of the corium, and the looser texture of the deeper or reticular layer, where the great bundles of fibrous tissue form rhombic and polygonal spaces by their intersection, which make on section a kind of lattice-work. There is also a distinct, though not uniform, arrangement of this tissue in successive layers, parallel to the surface. All this structure the author finds equally in the zones, which more or less perfectly encircle the limbs and trunk, and in the spaces between them. He then shows the arrangement of the hair-follicles, which are connected, not by their blind extremities, but by their edges, with four bundles of fibres running obliquely up from the reticular layer of cutis, and crossing each other obliquely before they expand in the papillary layer so as to surround the mouth of the follicle. This applies chiefly to the white fibres, as seen in tanned specimens of human skin. The elastic tissue is more irregularly disposed, while the inter-fibrillar cement (*Kittsubstanz*) pervades all parts alike.

The author does not find any continuous layer of endothelium lining the spaces into which the cutaneous lymphatics open.

The arrangement of the muscles of the skin is next described with great minuteness, and illustrated by figures of a mechanical model constructed for the purpose.

The second part of this paper deals with the blood-vessels of the integuments. The plan employed by the author was to inject first the veins and then the arteries with different coloured fluids, so that the place of meeting in the capillaries could be afterwards recognized. Professor Ludwig's apparatus was used; the injections consisted of size, coloured with solution of Berlin blue, watery solution of hydrated oxide of iron, dialyzed and afterwards concentrated by evaporation, or ferrocyanide of copper dissolved with oxalate of ammonia. Carmine injections did not succeed; the colours stained the surrounding tissues in spite of all precautions. Pieces of skin in which the injection had run well, from the face, arm, foot, hand, scrotum, trunk, &c., were hardened in alcohol: sections were made in various directions, and cleared with acetic acid and glycerine, or with turpentine.

The most important results of these observations, which are illustrated by numerous well-executed coloured drawings, are as follows:—

1. There is no direct communication between the cutaneous arteries and veins, as supposed by M. Sucquet; the capillary network is complete throughout.

2. There is no special capillary system for the fibrous and elastic tissue of the skin. Fine injections show that the capillaries are arranged as follows: (*a*) in the papillæ, where their function is supposed by the author to be the formation of epidermis; (*b*) around the convoluted part of the sweat-glands, and the sacs of the sebaceous and hair-follicles; (*c*) among the arrectores pilorum and the muscular fibres of the dartos; (*d*) surrounding the nerve fibres and the minute ganglioniform enlargements which have been described by the author;

(*e*) forming a somewhat loose and scanty network around the arteries —external vasa vasorum; (*f*) supplying the lobules of adipose tissue. Two points in the description of these capillary systems are worthy of special notice: one, that the sweat-glands are quite unconnected in their blood-supply with the papillæ, so that there is no second capillary network in the skin, as in the kidney; the other, that the special capillaries of the adipose tissue can be recognized in the fœtal integument before any fat-cells have been formed.

3. Three vascular layers may be distinguished in the human skin: a deep one, supplying the subcutaneous fat and deepest part of the corium, a mid-layer for the sweat-glands, and a superficial papillary network. The last discharges its blood into a venous plexus, which is almost erectile in character. The three corresponding sets of veins finally open into common collecting branches, visible to the naked eye.

4. The author confirms the observations of those histologists who have frequently met with papillæ which are at once nervous and vascular.

A New Sponge is described by Professor A. E. Verrill as having been obtained in the course of his recent dredgings on the coast of New England. He says* it is a large species, of which several fine specimens were obtained. This in general appearance and form somewhat resembles a *Tethya*, and in the character of the spicula it agrees with *Dorvillia* Kent. This sponge consists of a broad, convex, often nearly hemispherical, upper portion, two to four inches in diameter, supported on a broad, stout, but short, peduncle, usually two or three inches broad in large specimens, and somewhat less in height, the peduncle usually forming about one-half of the total height, which may be three or four inches. The peduncle is composed of very long, slender, irregularly aggregated, mostly setiform spicula, more or less appressed to the surface, but with the upper ends mostly free; together with a few small dependent fascicles. The "head" or upper portion of the sponge mass is firm and rather dense, composed chiefly of radiating bundles of large and long slender spicula, often more than half an inch long, many of which, at the external layer, divide into three horizontal or recurved branches or prongs, each of which usually forks near the end into two acute divergent branches, serving to support the cortical layer, which is more or less irregular and uneven, but firm; some of the spicula referred to project beyond the surface, and nearly the whole exterior is rudely and densely hispid, with long, setiform, acute spicules, which project unequally from the surface, the free ends of many of them being half an inch or more in length. Among the projecting spicula, and supported by them, are small, elongated oval, or fusiform, masses of soft sarcode, which are probably to be regarded as external gemmæ. Scattered irregularly over the upper surface, and especially around the periphery, are large, often very elongated, rounded, or angular, sunken areas or pits, often half an inch across, surrounded by a more or less prominent margin sup-

* Silliman's Journal, May, 1874.

ported by stiff projecting spicula. The bottom of these pits is formed by a thin membrane or diaphragm, perforated by very numerous small round or oval openings, which are quite variable in size, even in the same area, and in many cases are so numerous and large in the central part as to be separated only by a mere network, when they become polygonal. This perforated membrane is filled with minute, many-rayed double-stellate spicula, with a small number of much larger ones having four or five acute rays. Beneath the diaphragm the pits become more or less funnel-shaped, and communicate with large round anastomosing channels, which ramify through the sponge-mass.

It seems necessary to refer this remarkable form to the genus *Dorvillia*, and therefore he proposes to consider it a new species under the name of *Dorvillia echinata*.

Striated Muscular Fibre.—At a recent meeting of the Boston Society of Natural History, Dr. Thomas Dwight read a paper on the "Structure and Action of Striated Muscular Fibre." His studies had been made on the muscles of the legs of the small water-beetle *Gyrinus*. Their covering is quite transparent, and after the leg has been cut off and put into a drop of water under a covering glass, the contractions can often be observed for over an hour. He found that the fibre, at rest, consisted of narrow granular transverse stripes, with broad light-coloured bands between them. Close to the black stripe there was a glaring white reflexion, but midway between two stripes the fibre was grey. When the fibre contracted the black bands came nearer together, and their granular structure became more obscure; the grey band disappeared, so that there was merely an alternation of black and white stripes. The ends of the white stripes bulged out during contraction. As the wave of contraction moved along, it was easy to see that there was no interchange of position between the black and the light substances, and no homogeneous transition stage, as is maintained by Merkel. When one part of the fibre is in contraction, the part from which the wave is running is put upon the stretch; the black bands are divided into two rows of granules, and there is less distinction between the white and grey substances.

The Capability of the Microscope.—According to the researches of Professor Abbe, published in a late number of Max Schultze's 'Archiv,' and abstracted by one of our contemporaries, it is made to appear that the limit of capability of the microscope is almost reached by our best microscopes, and that all hope of a deeper penetration into the material constitution of things, than such microscopes now afford, must be dismissed. [!!] Experiment and theory agree in showing how the changes wrought by diffraction of light passing through fine structures, whose elements are so small and near each other as to call forth this phenomenon, are such as to prevent the object being imaged *more geometrico.* Thus it may happen, on the one hand, that different structures give the same microscopical image, and, on the other, that like structures give different images. Consequently, while objects of the kind (systems of fine lines and the like) may appear ever so distinct and well marked in the microscope, we are not entitled to regard such

appearances as of morphological significance, but merely as physical phenomena, from which nothing further can certainly be inferred than the presence of such structural conditions as are capable of producing the diffraction effects obtained. The remark has notable applications to many of the microscopical researches on markings of diatoms, and on striated muscular fibre. And it affects not merely the morphological relations of the objects, but the deductions, made from microscopical observation, as to properties (such as differences of transparence, colours, polarization, &c.). The author lays down the following principle as basis for determination of a limit:—By no microscope can parts be distinguished (or the marks (*Merkmale*) of a really present structure perceived), if they are so near to each other that the first bundle of light rays produced by diffraction can no longer enter the objective simultaneously with the undiffracted cone of light. Professor Abbe has also recently described a new illuminating apparatus for the microscope, formed of a condensing system of two unachromatic lenses, which are fixed in the stage of the microscope, and transmit the rays from the mirror below; the purpose being that the object (immediately above the upper lens) may be illuminated by light from a great many different directions.

Is Eozoon Canadense a Foraminifer or not?—This important question which was long ago discussed by Dr. Carpenter and Professors King and Rowney, has recently been taken up by no less an authority than Mr. H. J. Carter, F.R.S., who alleges that it presents none of the features of an animal; he has been replied to in a very able paper by Dr. Carpenter, in the last number of the 'Annals of Natural History.' We propose to lay the former view of Mr. Carter before our readers in the present number, and Dr. Carpenter's in the next number of this Journal. After giving the structure of certain fossil specimens of *Nummulites* and *Orbitoides*, he goes on to say, that "in vain do we seek in the so-called *Eozoon Canadense* for the unvarying perpendicular tubuli, *sine quâ non* of foraminiferous structure. In vain do we look for that regularity of chamber-formation which, in the amorphous growth assigned to the so-called *Eozoon*, might be equally well assumed to be identical with the heterogeneous mass of chambers on each side of the central plane of *Orbitoides dispansa*, accompanied by the transverse bars of stoloniferous structure uniting one chamber to the other. In short, in vain do we look for the casts of true foraminiferous chambers at all, in the grains of serpentine; they, for the most part, are not subglobular, but subprismatic. With such deficiencies, I am at a loss to conceive how the so-called *Eozoon Canadense* can be identified with the foraminiferous structure, except by the wildest conjecture; and then such identification no longer becomes of any scientific value. Having examined the slice of Laurentian limestone which has been so courteously submitted to me, in thick and thin polished sections, mounted in Canada balsam, by transmitted and also reflected light, also the surface of the 'decalcified' slice as it came from you, in all directions, with one-quarter and one-inch focus compound powers respectively, I must unhesitatingly declare that it presents no foraminiferous structure anywhere. Nor does its structure

bear so much resemblance to that of a foraminiferous test as the legs of a table to those of a quadruped; while, if such be the grounds on which geological inferences are established, the sooner they are abandoned the better for geology, the worse for sensationalism! The contents of this letter are open to no controversy. My knowledge of foraminiferous structure has been obtained step by step, beginning with the recent and then going to the fossilized forms, making and mounting my own sections, from which afterward my illustrations and descriptions have been taken. If others who have pursued a similar course of instruction differ from me in what I have above stated, the question can only be decided by a third party, not on verbal arguments alone, but on a comparison of the actual specimens, as prolonged disputation, in matters of opinion, soon disgusts everybody but the combatants, and can end in nothing but a fearful waste of time that might be better employed."

The Air-cells in Limnanthemum. — Dr. T. G. Hunt contributes a short account of the above to a recent number of the 'American Naturalist.' He says that in the leaf of *Limnanthemum lacunosum*, or floating-heart, may be demonstrated multitudes of peculiar stellate bodies, apparently like those found in the stem of *Nuphar*. The whole interior of the leaf is studded with them. There are no ordinary large air-spaces so often found in other floating-leaves, but all through the parenchyma these curious bodies are irregularly scattered. They vary in size and also in the number of rays given off by each. These rays are smooth and not echinulate like those in *Nuphar*. In the field of a $\frac{2}{3}$ lens he has counted hundreds at one view. Under the polarizing binocular microscope, properly illuminated, they are revealed with startling distinctness and beauty. It is nearest the under epidermis that they are located, and the best view therefore is obtained from beneath. Their true physiological significance is not doubtful. In the natural condition they contain air, and the floating-heart rides securely on the surface of the lake, buoyed up by innumerable life-preservers which are not likely to shift out of place. The veins in the leaf are present, of course, but are comparatively rudimentary. The vascular bundles are faintly marked, and only a few delicate supporting cells line their margins; thus giving another example of nature's economy, for where strongly developed organs are not necessary there we do not find them.

Mr. W. Archer on the so-called Ague-plant. — It seems that the editor of 'Grevillea' sent some of this plant to Mr. Archer, of Dublin, who has explained its nature very distinctly by showing that it is simply *Hydrogastrum*. He says (in 'Grevillea,' May), "On reading over the more recent description of the 'Ague-plant,' communicated by Dr. Bartlett to the 'Chicago Society of Physicians and Surgeons,'* one sees how fairly it tallies with the known characters of Hydrogastrum,† but it is undoubtedly surprising how he and the American observers of the Society referred to (*loc. cit.*) failed to perceive the identity of the

* See 'Grevillea,' No. 21, March, 1874, p. 142.

† See also Parfitt in 'Grevillea,' No. 7, January, 1873, p. 103.

organism in question, one which finds a place in so many botanical text-books, both by figure and description, as well as on lecture-diagrams, as a noteworthy example of a single-celled independent plant, and at the same time endowed with the power to become copiously ramified, so to speak, 'root,' 'stem,' and aërial portion combined in one 'cell' only. I venture to think it hardly less surprising to find this seemingly so passive and inert little chlorophyllaceous alga, met with, in suitable situations, all over Europe, gravely *tried and found guilty*, on so slender evidence, of being the atrocious 'cause of the ague.'

"In the mud-samples so kindly forwarded by the Editor, there occurred some fragmentary examples of a plant wholly different from the foregoing—so small in quantity as to be quite invisible to the unassisted eye—but which disclosed itself amongst the *débris* taken up along with the Hydrogastrum. This was a Chthonoblastus, Kütz. (Microcoleus, Harvey), and was most probably the same as *Ch. æruginеus*, Kütz. Just where one of these algæ would be found it would not be very surprising to meet with the other. Can this latter be chargeable with being the 'cause of the ague'? It is wholly a different kind of alga from Hydrogastrum, without any point of homology or affinity therewith, except, perhaps, their common love for the damp clayey substratum afforded by the partial drying of the swamps, near which, unfortunately, from some occult cause, the 'ague' is prone to hover."

The Mucous Membrane of the Larynx.—Dr. W. Stirling contributes a note on this to a recent number of the 'Medical Record.' He states that Mr. P. Coyne has arrived at the following conclusions upon this subject. The mucous membrane of the larynx is formed in a layer subjacent to the epithelium, by a reticulated tissue analogous to lymphatic tissue; it thus approaches the structure of the mucous membrane of the small intestine. Lymphatic organs, analogous to the closed sacs of the small intestine, exist in the superficial layers of the mucous membrane. The author is of opinion that the presence of these glands may account for the development of certain ulcerations in the larynx during fever, as in typhoid. On the free border of the inferior vocal cord certain vascular, and probably nervous papillæ, are to be found. These papillæ are specially developed on the anterior half of the vocal cords. From the preparation of more than twenty-five human larynges, the author has satisfied himself that the sub-mucous serous sac, admitted by Fournié, does not exist.

The Nerve of the Digestive Canal.—A paper on this subject which, though short, is not devoid of interest, appears in the 'Medical Record,' April 29th. It is really an abstract of Professor Arnstein's communication of the results obtained by Professor Gonjaens. It is to the following effect:—

1. Ganglion cells occur in considerable quantities in the walls of the œsophagus of the frog.

2. The nerve-stems of the mucous membrane of the œsophagus of the frog lie generally in lymph-spaces. The fine nerve-fibres, devoid of the white substance of Schwann, which branch from these nerve-

stems, run within the *Saftcanäle* between the bundles of connective tissue.

3. A thick net of non-medullated nuclei containing nerve-fibres is distributed in the mucous membrane of the œsophagus of the frog. From this net, fine nerve-fibres branch off and ascend vertically in the direction of the epithelium, and can be followed to the interstices between the epithelial cells.

4. The nerve-fibres of the mucous membrane of the stomach and intestine of the frog arise from nerve bundles, which often pierce the muscular coat in company with small arteries. In their further course, two principal directions are to be made out. Part of the fibres ascend vertically from the deeper layers of the mucous membrane and reach the epithelium of the mucous layer. In this course, they give fine threads to the gastric and intestinal glands. A connection between the nerve-threads and the epithelial structures could nowhere be made out. The other part of the nerve-fibres of the mucous membrane ascend at first in the form of an arch, and later have a direction parallel to the surface of the mucous membrane. In that the above-named bendings occur at different heights, so that on a vertical section there appear three or four parallel rows of nerve-fibres. These nerve-fibres are destined for the capillary vessels of the mucous membrane, and form, on making sections parallel to the surface, long drawn-out nets around the blood-vessels; single nerve-threads touch in a radiating manner the walls of the capillaries.

Is Dichœna rugosa a Lichen or Fungus?—This subject is very well discussed in a paper by Mr. F. C. S. Roper, F.L.S., which was lately read before the Eastbourne Natural History Society. After some introductory remarks, the author said the whole surface of the patch when viewed with a low power is granular or minutely tubercular, of a dull brownish-black colour, and in some cases tinged here and there with green. On making a section, it is found that these granular bodies arise from below the cuticle of the bark, but without any trace of mycelium penetrating the bark itself, as is commonly the case with Fungi; they are of irregular outline, and enclose a cavity, opening by a pore, or rather slit, at the summit. This cavity is more or less filled with asci, or small sac-like bodies, which contain spores, surrounded with a mass of filaments called paraphyses, which are septate or jointed, and curiously bent or hooked at the summit. The number of spores in each ascus is variable, generally from two to six, though as the spores are large, it is not improbable that the normal number is eight. The spores are oval, and the largest about $\frac{1}{1000}$th of an inch in length; they are filled with granular matter, of a pale brownish tinge, variegated by a mixture of bluish green. These spores, when mature, escape through the aperture at the summit of the granules. Scattered over the edges, and at times imbedded between the plants, we find masses of green bodies (Gonidia), which are very minute, varying in shape from distinct circles to ovals or oblongs, encircled by a hyaline or transparent border with a double-cell wall. The green matter in the smaller of these bodies (which is probably chlorophyll) is homogeneous. In the larger we find segmentation commenced, and

they are divided into two, four, or eight masses, separated by a distinct partition, but still enclosed in the hyaline cell wall. It must be remembered that each of these granular brownish-black bodies is a separate plant, and it is only by their rapid increase and aggregation into masses that the patch we see on the tree is produced. It is a matter of considerable difficulty to convey in writing a clear description of these minute forms of life; but I trust, by the help of the specimens and drawings on the table, I have made sufficiently clear all the data we have to enable us to find out its position in the vegetable kingdom. I may, however, mention that no reaction is found on the contents of the perithecium either by potash or iodine.

The question first arises, after having ascertained its structure, Where are we to look for it? The general appearance is certainly that of a Lichen, and the black oblong or ovoid perithecia have much the appearance of some of the Graphideæ. The spores and asci give us no help, as they may belong either to a Lichen or a Fungus. The great difficulty arises from the presence of the green particles, termed gonidia, which are identical with some of the Algæ, and have been figured and described under various names by Kützing, Hassal, and others, as separate and distinct plants; but similar bodies are also found in Lichens; and we find the Rev. M. J. Berkeley, one of our greatest authorities, in his introduction to Cryptogamic Botany, defines Fungi as "plants Hysterophytal (that is, living upon dead or living organic matter) or Epiphytal (that is, growing upon another plant), nourished by the matrix, *never producing gonidia;*" whilst his definition of Lichens is "Aërial, nourished by air, and not by the matrix, *producing gonidia.*" Of course, with these definitions, anyone would naturally expect to find our plant amongst Lichens; but a most careful examination of Leighton's 'Lichen Flora,' the latest and best work on this tribe, together with Mudd's Manual and other works, failed to show me any description that would agree in all respects with the appearance shown by the plant I have described. I then asked Mr. Muller to examine it, and ascertain if it could be a Fungus. The mixture of distinguishing characters was as great a puzzle to him as it had been to me, and he was unable to make out from Cooke's Handbook its exact position; but on carefully going through the ascomycetes Fungi in Hooker's 'English Flora,' he found *Hysterium rugosum* described as "Stroma, crust-like, innate, brown-black, perithecia elliptic bursting through the living bark, at length running together into irregular spots." This is said to be extremely common on the smooth branches of birch and oak. And Mr. Berkeley, who prepared this portion of the British Flora, states also that it is usually referred to the order Lichenes, from which, however, Messrs. Borrer and Hooker, in accordance with the views of Chevalier, Wallroth, and Fries, consider it extraneous. Sir James Smith long since perceived its affinity with Hysterium, from which it differs in the presence of a stroma, and in its being produced on living bark. Reference is made to 'English Botany,' t. 2282, and on looking at the figure and description there given, as well as to the works of Fries, Acharius, and other authors, it was evident this was undoubtedly the plant we were in search of. The

synonymy is curious, and well exemplifies the difficulty cryptogamic botanists find in clearly defining the limits of these lowly-organized plants; for I find that ten well-known authors describe it as a Lichen, and six, equally well known, place it amongst the Fungi; whilst it is rejected by both our latest authors on these plants, Mr. Cooke, in his 'Handbook on British Fungi,' 1871, merely mentioning the name of *Dichæna rugosa*, with the remark, "I think it should be included with Lichens," and the Rev. W. A. Leighton, in his 'Lichen Flora,' published the same year, taking no notice of it whatever.

Pseudo-Muscular Hypertrophy. — The 'Philadelphia Medical Times' contains a translation of an article on this subject. After describing the usual symptoms and course of the disease, and the microscopic appearance as being simply an atrophy of the muscular fibre, accompanied by an enormous increase of the interstitial fatty and connective tissue, the writer passes on to the consideration of four cases, which, though presenting some points of similarity, were in others markedly different from the ordinary course of the disease. In each of these cases the disease was consequent upon an injury. The functional derangements were not so marked as in typical cases: in place of being totally lost, the power of motion was only diminished. The microscope revealed, in each case, what appeared to be a true hypertrophy of the muscular fibres, without excessive growth of the interstitial connective tissue. From this it would seem that the hypertrophy spreads from the muscle to the connective tissue, and the hypertrophied connective tissue, pressing on the muscle, causes atrophy afterwards. Schlesinger, however, reports a case of a man with mental disease, in whom some of the muscles were hypertrophied. The microscope showed the muscular tissue much diseased, but in them there was no hypertrophy of the muscular fibre. Whether the process is a simple inflammation, or what is its nature, is not known. —See also the 'Medical Examiner,' Chicago, May 1.

The Microscopy of Gum Production has been fully explained in a paper before the French Academy by M. Prilling. The writer divides the subject into three heads, as follows :—

1. *Production in vessels.*—In the wood of a tree so diseased as to produce gum, a large number of vessels are more or less filled with it either through their entire length or forming a coating more or less thick around them, or on one side. The most recent observers have admitted that the gum results from the disorganization and transformation of the inside of the walls of the vessels, but an attentive study of the production of gum in the vessels has led me to a different conviction. The gum shows itself first in very fine drops, which increase in size, touch each other, become confluent and form irregular masses, with sinuate edges. This mode of origin of gum contained in vessels appears irreconcilable with the opinion professed by German observers. The examination of large masses of gum taken from the vessels of the apricot has led to the same conclusion. These vessels are marked with areolar cavities and a spiral projection formed by a thickening of the cell-walls on the interior, and the masses of gum

present on their surface furrows corresponding to the spiral lines which jut out from the walls of the vessel, and even little projections corresponding to the cavities. It is then very certain that, in this case, the gum is deposited in the interior of the vessels, and has taken the impression of the interior. This gum is of the same nature with that M. Trécul calls *cérasone.*

2. *Production in Cells.—Transformation of Starch.*—Gum is often seen in the medullary rays and there offers particular interest, because its appearance is connected with the disappearance of the starch originally contained in the cells. The change of starch into gum has been noted by former observers, but never to my notion precisely described. On the first appearance of gum in the cell, the grains of starch, still entire, are gathered into little groups, around which appears a thin layer of gum, also small portions of which may be seen deposited in other parts of the cell. The masses of starch enveloped by gum diminish continually as the layer of gum increases in thickness, but when treated by iodine the two substances preserve their special properties without modification till the starch finally disappears, usually leaving a small cavity in the centre of the little mass of gum. When the production of gum commences in the tissues, an increased amount of starch is observable in the neighbouring cells which seems absorbed, and immediately changed into gum, but ordinarily the gum in this case does not appear to be deposited in the cells, but passes into the neighbouring reservoirs, where it accumulates in considerable quantity.

3. It is neither in vessels nor cells, but rather in the lacunæ formed in the interior of young tissues, the voluminous masses of gum accumulate, which we often observe. These lacunæ are most frequently found in the cambial zone, but may be seen at different depths in the wood, disposed concentrically like successive annual layers. They are formed in the germinal layer, and then occupy the interval between the medullary rays. When not too largely developed, a new woody layer forms outside of them, and the growth is not sensibly altered. On the contrary, if growth cease at this point, a flow of gum is caused, the woody tissue necroses and cannot be covered except by the extension of lateral portions where the germinal layer is uninjured.

The tissues next to these lacunæ suffer an important modification of development; the cambium, instead of forming woody tissue, produces cells in which an abundance of starch is deposited. There arises then, wherever gum is developed, a particular tissue (woody parenchyma) which does not exist in healthy stems, and whose appearance is so intimately connected with the morbid formation of gum, that it may be considered as a pathologic tissue. The starch, which accumulates in this special woody parenchyma, is used, as in the medullary rays, to form gum, which accumulates in large quantities in the lacunæ. These lacunæ increase at the expense of the neighbouring tissue, which is disorganized; nevertheless, the cells which border the lacunæ often manifest extreme vital activity, and give birth to true pathologic formations. They develop, multiply and ramify in the interior of the lacunæ, even when separated from the rest of the tissue, and absolutely isolated in the middle of the gum.

What is the exact Definition of Leucocyte and Pus-corpuscle?—The following is an account of an amusing discussion which took place at a late meeting of the Philadelphia Pathological Society. "Dr. Bertolet said he had not a clear idea of what was comprised by the term '*leucocytes*,' and desired much to know its limitations. Dr. Tyson replied that, after other better-known histologists, he had always used the word *leucocyte* in a generic sense, as including all that class of small, round, variously granular cells which, according to the situations in which they were found, were variously called white blood-corpuscles, mucus-corpuscles, young pus-corpuscles, or the round cells of connective tissue,—in other words, *dead amœboid cells*. Dr. Bertolet said he thought this was an error in theory which had been allowed to supplant practice; that the white corpuscle and pus-corpuscle were not the same. Dr. Richardson said the word *leucocyte* had been originally introduced by Charles Robin, who applied it to the class of bodies named by Dr. Tyson, whether alive or dead, as well as to exudation-corpuscles, and he believed also, provisionally, salivary corpuscles. He thought that if anyone would treat white corpuscles contained in a drop of blood from his finger, first with water by introducing a small quantity at the edge of the thin glass cover, and then with weak aniline solution, in the manner described in his report on the white blood-corpuscle,* he would have no difficulty in finding many globules which exhibited two or three, and occasionally those which displayed four or five, well-formed and strongly-tinted nuclei, and which manifested a precise identity, in that respect at least, with the leucocytes of pus, as described by older pathologists. By this experiment it was easily demonstrated that the characteristic formerly so much relied upon for the recognition of the pus-cell, and quoted by Dr. Bertolet,—namely, that it possessed two, three, or more nuclei,—was valueless as a means for its discrimination from the leucocytes of blood. Dr. Tyson admitted that pus-corpuscles soon became very granular from fatty degeneration, and then presented objects which did not so closely resemble the white blood-corpuscle; but in their young state he did not think they could be distinguished, and to acetic acid and water both responded identically. Dr. Richardson said that about one white corpuscle out of thirty is ordinarily more granular than its companions, and he was strongly inclined to think that these white corpuscles were also the seat of fatty degeneration. The President said it was very important to have clear ideas as to the exact application of terms. He presumed, of course, that this discussion referred simply to the morphology, and not the vital properties or developmental tendencies, of the cells in question. He said that he himself had been called upon to study cases where inflammation had obliterated the trunks of vessels,—a matter which brought up directly the question of being able to distinguish between the corpuscles in the surrounding inflamed tissue and the white corpuscles which remained in the softened clots. By no means which were available could he distinguish between the two. Dr. Richardson thought the more he studied the subject in connection with Cohnheim's observations, the more he was led to conclude that living leu-

* 'Amer. Med. Assoc. Trans.,' 1872.

cocytes of pus and blood were identical physiologically as well as morphologically."

The Minute Structure of a Peculiar Fern.—At one of this year's meetings of the Philadelphia Academy, Dr. J. G. Hunt remarked that the structure of the *Schizæa pusilla* differed widely from that of our other indigenous schizæceous ferns, *viz.*, *Lygodium palmatum*, and its morphological elements are unlike those of our ferns in general. The barren frond of *Schizæa pusilla* is marked on its epidermal surface with a *double line* of stomata, and these organs extend the entire length of the frond. The cells which make up the interior of this delicate fern are cylindrical and vary in size, but their distinctive characters lie in minute projections or outgrowths from all sides of the cells, and these projections meet and are articulated with corresponding outgrowth from adjoining cells, so that the cells of *Schizæa* have penetrating between them in every direction intercellular spaces and channels of *remarkable regularity* and beauty, and so characteristic is this plan of cell-union that the botanist need find no difficulty in identifying the smallest fragment of the plant. This morphological peculiarity has not been noticed before.

Microscopic Crystals.—These have formed the subject of a couple of papers by Dr. Lea in the 'Proceedings of the Philadelphia Academy of Natural Science.' They are illustrated by a plate. The minerals examined were garnets, asteriated sapphire, labradorite, a black feldspar, barite, amethyst, ruby.

NOTES AND MEMORANDA.

Photographs of Microscopic Writing.—The following account is given by Colonel Woodward in a letter to Mr. Ingpen, F.R.M.S., the Secretary of the Quekett Club, and is published by him in the Journal of that Society:—"Two samples of Mr. Webb's fine writing on glass have been received at the Museum since my communication of August 18th. Each consists of the Lord's Prayer, written with a diamond, according to the label, in a space $\frac{1}{294} \times \frac{1}{441}$ of an inch. In one of the slides the writing is blackened, and mounted in Canada balsam; in the other it is not blackened, and is mounted dry. I send photographs of both herewith — the one magnified 650 diameters, the other 825. I find Mr. Webb's statement of the dimensions in which this writing is executed to be substantially correct, and he has certainly produced a most curious and interesting object for microscopical study. To compare his work with the coarser bands of Nobert's plate, I took a photograph of the first seven bands of the Nineteen-band plate with 650 diameters, which I also forward herewith. This photograph, and that of the blackened writing, were taken on the same day with the same objective, Powell and Lealand's immersion $\frac{1}{8}$th, at the same distance, and under identical conditions.

The photograph of the writing was made first, and is the best of a number of trials. I then inserted the Nobert's plate, not even changing the cover correction, as I should have done to secure the best definition, because this would have changed the power. The picture sent was the result. A comparison of the two pictures will render any remarks on the relative delicacy of Mr. Webb's work and that of Nobert unnecessary. It is evident that the point used by the former is very much coarser than that used by the latter. The picture of the Prayer, mounted dry, was taken on a subsequent occasion, and is also the best of a number of trials. It is taken with the same objective as the other pictures, but with a different cover correction, and somewhat greater distance. Both the samples sent me by Mr. Webb are inscribed on such thick covers that they are seen under a disadvantage, and my highest powers cannot be used on them. The writing is, however, comparatively so coarse that it can hardly be considered as a serious test for high powers. Either plate is easily read with a good half-inch objective and central light. I am curious to learn how this writing of Mr. Webb's compares with that of Mr. Peters, described by the late Mr. Farrants in his address as President of the Royal Microscopical Society. He stated that it was executed at the rate of twenty-two Bibles to the inch. I would greatly like to see such a specimen, and give it a photographic trial. Will you kindly read this note to the Club, and present the photographs? I send also a full set of my last photographic analysis of Nobert's plate for the Club, and a package for Mr. Webb, which I beg you to hand him."

A Spherical Diaphragm is thus described in the 'American Naturalist' by Mr. F. B. Kimbal. He says:—"Wishing to use tubular diaphragms with my microscope, and knowing how clumsy the ordinary ones are, I set to work, and endeavoured to devise a substitute. I made a globe 1¼ inch in diameter, and drilled holes through it of the proper grade of sizes, and adjusted it so that by a spring stop the holes will correspond to the axis of the microscope when the ball is revolved on its axis by a milled head at the right of the stage. The fittings are so arranged that the diaphragm may approach or recede from the stage so as to touch the slide or be far from it. The globe may be made hollow and the lower part cut off if the tubular wells are not desired. I think this form of diaphragm offers many advantages over the ordinary piece of apparatus."

A New Microscopical Society has been formed at Louisville, Kentucky, U.S.A., which meets the first and third Thursdays of each month. The following are the names of the officers for the ensuing year:—*President*, J. Lawrence Smith; *Vice-Presidents*, Noble Butler, Chas. F. Carpenter; *Treasurer*, C. T. F. Allen; *Cor. Secretary*, E. S. Crosier; *Secretary*, John Williamson; *Executive Committee*, Thos. E. Jenkins, James Knapp, W. T. Beach, E. R. Palmer, R. C. Gwathmey.

CORRESPONDENCE.

Who Sent Mr. Tolles' Objective to Mr. Crisp?

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *May* 19, 1874.

Mr. Editor,—I find in the May number of this Journal that the Rev. Mr. Brakey has indulged again in his favourite pastime of describing the impossible, *viz.* the thoughts and motives of persons three thousand miles* distant across the Atlantic Ocean. One wonders if his clairvoyant medium is reliable?

He writes, "Mr. Tolles had constructed an objective which he labelled with the astonishing angle of 180°, and not only constructed it, but in an evil hour sold it to Mr. Crisp, little *thinking* that in so doing he was selling himself into the hands of the Philistines, to be shown and made sport of."

The truth is that Mr. Tolles had little to do with the sending of that objective to England. It was sent by myself of my own motion, expecting that some of the Philistines would break their heads against it in their "sport"; and with a special request that it might be seen by the most unbelieving Philistine of the Philistines—Mr. Brakey himself! Mr. Brakey should be acquainted with the history of the Philistines, and may take warning by their fate after they aroused Samson.

I may add that the objective was not purchased by Mr. Crisp until after he had seen it.

In the future Mr. Brakey should be more cautious of writing what he cannot possibly know, and then he may not so often be put into such "ludicrous" positions.

Charles Stodder.

Mr. Pillischer's Reply to Mr. Brooke.

To the Editor of the 'Monthly Microscopical Journal.'

8, Lower Rock Gardens, Brighton, *June* 19, 1874.

Sir,—Having been indisposed and away from home, Mr. Charles Brooke's letter in the April number of this Journal, in reply to mine in the March number, has only just come to my notice.

Being still far from well, I should willingly allow Mr. Brooke's letter to pass unnoticed, but for the glaring misrepresentation it contains, which in order to refute, I have once more to ask your favour of inserting this in the next issue of your valuable Journal, after which I shall consider, as far as I am concerned, this matter at an end.

Mr. Brooke is astonished at the tone of my letter, and says: "First, as to nationality: my authority was a juror at Vienna," and regrets

* Mr. Brakey says 2000 miles. His readers may wonder if his knowledge of optics is any more accurate than his knowledge of geography.

having erroneously repeated it. I assure Mr. Brooke that beyond the questionable propriety of introducing my nationality at all in his address to the members of the Royal Microscopical Society, to whom I am by no means a stranger, I care not a button whether he believed that I was a Prussian or a Turk.

With regard to Mr. Brooke's argument that native British optical goods were wholly unrepresented at the Vienna Exhibition, I think any schoolboy, if asked, will tell him that goods manufactured in England, of English materials, by English workmen, under the superintendence of an English foreman, are to all intents and purposes native British products.

As to deep objectives: somehow it pleases Mr. Brooke to remember my telling him at the Exhibition that I had no higher power than a $\frac{1}{2}$-inch, but I expected some, and he attributes to me extreme carelessness for not informing him personally of their subsequent arrival. I emphatically deny in the most positive terms having had any conversation whatsoever with Mr. Brooke on the above subject, except on the day when my microscopes and objectives were before the jury; it was there, I repeat, that he asked me what objectives I had to show, and when telling him that a series ranging from 4-inch to $\frac{1}{25}$-inch were on the table, he replied, "I do not care for high powers," and left it to me to show him what I liked. The day having been dark and gloomy, and no artificial light provided, and convinced that under such circumstances it would be vain to attempt to show a high power; moreover, perceiving that the only test the jury had at their disposal was a coarse *angulatum* intermixed with a specimen or two of *Surirella gemma*, I contented myself with showing the *angulatum* under a $\frac{1}{8}$-inch and Kellner's D eye-piece, evidently to the satisfaction of the three jurors present.

Apologizing for trespassing on your valuable space,

I remain, Sir, your obedient servant,

M. Pillischer.

[Out of a desire to exhibit fair play we have inserted Mr. Pillischer's letter, having removed the more objectionable passages. At the same time we cannot but deprecate the tone of his observations. On communicating with Mr. Brooke, he has informed us that his "recollections of Vienna are entirely at variance with those of Mr. Pillischer."—Ed. 'M. M. J.']

Immersion *v.* Dry Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

1, Bedford Square, *June* 20, 1874.

Sir,—I have no intention of asking you to devote more of your valuable space to "The *battle* of the Lenses," nor am I at all inclined to enter into a controversy with the Rev. S. L. Brakey, "On the Theory of Immersion," but if Mr. Brakey's practical experience of the immersion system is too limited to enable him to say whether or not "the immersion lenses do actually possess the superiority of definition which has lately been ascribed to them," I venture to think

he will do well to visit some of our Metropolitan Medical Schools, or the Medical Microscopical Society, where I know he would have ample opportunities afforded him of both seeing and learning that the immersion system has much simplified the whole process of obtaining high-power definition; and that students are now able to examine for themselves with a magnification of a thousand diameters, where formerly such magnification was scarcely practical, or only known as of difficult achievement.

Doubtless, if Mr. Brakey could see a fair comparison made between immersion objectives, as those of Hartnack, or some other equally well-known maker, and the old or even modern dry objectives, he would at once admit that there is not much room for a play of fancy, "as in judging the merits of wine," and he might "exactly say how much of the difference is really due to the immersion system," both as to practical results obtained, economy of time (probably also of money), and what is due to a previously settled "conviction."

I have the honour to remain,

Your most obedient servant,

JABEZ HOGG.

ON IMMERSION LENSES.

To the Editor of the 'Monthly Microscopical Journal.'

SIR,—As an Immersion of some years' standing I should like to say a few words on behalf of myself and of certain of my fellow-objectives.

Your contributor on the Theory of Immersion suggests that we have received undue praise for our merits, which he says are so indefinite that he does not care to pledge himself either for or against us; he evidently inclines to the belief that the qualities claimed for us, and on which we pride ourselves, are mainly imaginary.

I think your contributor might well admit that not everyone, save himself, who has seen and examined, is still unconvinced of our high qualities of clearness and definition. In this I would ask him to believe it possible that others besides himself and Mr. Wenham have given serious attention to the subject of Immersion, and that some have had quite as much and possibly more varied experience than either of them in the use of Immersion lenses. I venture to say that neither of them would presume to be of greater authority on the Theory of Optics than Sir David Brewster or Amici, both of whom declared in favour of the Immersion principle. Sir David (then Dr.) Brewster, indeed, claimed to be the inventor of the first compound microscope involving the Immersion principle: he describes it in his 'Treatise on New Philosophical Instruments,' published at Edinburgh in 1813: while Amici not only spoke in favour of the principle, but exhibited Immersion lenses made by himself, which he openly stated would show minute structure that was invisible to the Dry Objective. Since then, certain Paris opticians and others have given special attention to the Immersion principle, and have not been sparing of

their criticism of the way in which the English Jurors passed over their work at the Paris International Exhibition. I well remember being there myself, but because the Test-object I was exhibiting was unknown to the said Jurors, I was scarcely noticed by them. I heard too, that an English optician criticised us with some acrimony in the 'Athenæum'; he went so far as to say that the Test-object, which I was exhibiting with a magnification of about twelve hundred diameters, was altogether too coarse to use as a test even for a low power;—but he was beside the mark.

Very soon after this, myself and other Immersions were pitted against some of the then best Dry Objectives in England, and we more than held our own. The merits of our principle of construction being thus brought prominently to the notice of English amateurs and opticians, some of the latter were not slow in taking the matter up. Then came the announcement that Dr. Woodward had made a series of photographs of Nobert's new Nineteen-band Test-Plate with Messrs. Powell and Lealand's $\frac{1}{16}$th Immersion. This was immediately followed by copies of the photographs which were exhibited at the Royal Microscopical Society. On comparison with the photographs of the same Test-plate made with some of the finest and most powerful Dry Objectives it was abundantly evident that the Immersion principle would now take the lead. Dr. Woodward, with a zeal and perseverance that were truly admirable, followed up these photographs by others of the *Podura*, *Amphipleura pellucida*, *Frustulia saxonica*, *Rhomboïdes*, &c., &c., produced by various Immersion lenses; and these photographs can be referred to in the collection of the Royal Microscopical Society by any one interested in the subject.

So far as I am concerned personally I shall be glad to have our merits judged by our works. On the part of many of my fellow Immersion lenses and myself I accept Dr. Woodward's photographs as fairly representing our capabilities up to the date of their production; and I venture to believe that in them we show results ahead of anything that has hitherto been done of a similar kind by any Dry Objective.

Since that date, and notably as exhibited at the Vienna Exhibition, improvements have been made in our construction; our younger brethren have had a fourth combination added; that is, have a single front (which the oldest of us have had — aye, even that made by Dr. Brewster in the beginning of the century;—I hope Mr. Wenham will pardon this slur on his claim to be the inventor of single fronts!) and three doublet achromatics progressively increasing in diameter. In this construction a zone of peripheral rays is gained and made available in the formation of the image;—which we believe will be an advantage in high powers.

The close study of M. Pouillet's experiments to determine the conditions of the production of diffraction in the passage of light through various forms and parts of prisms has led one of the most learned of the Paris opticians to aim at extending as much as practicable the introduction of marginal rays into the formation of the image: the new objectives of four combinations that were shown at the Vienna Exhibition were practical examples in this direction.

In the meantime your contributor has touched on an important point in stating that our excellence (which he grudgingly says may be assumed for the present) is partly due to the greater intensity of the rays of light we transmit. He may now forestall the Paris *savant*, in whose conversation the statement and demonstration of these investigations has formed an integral element during ten years to my knowledge, and show experimentally and mathematically that the theory of the production of the minutest optical images requires the greatest possible preponderance of peripheral over central rays in the objective: that the immersion principle greatly assists in the attainment of this condition: that we thus inherently possess greater freedom from errors of diffraction that necessarily exist in the Dry Objectives.

At this date, with lenses already made on the Immersion principle of focal lengths varying from $\frac{1}{4}$th to $\frac{1}{80}$th of an inch, we do not ask to have our merits *assumed;* we point to *our* series of Dr. Woodward's photographs as conclusively proving that we possess photometric powers and other qualities most highly valued in microscopic definition, in a degree quite beyond those of Dry Objectives.

I am, Sir, your obedient servant,

Immersion Lens.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *June* 3, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting was read by the Secretary, and the thanks of the meeting were voted to the donors.

The President announced that the reading room and library of the Society would be closed during the month of August. He also regretted to inform them that a paper which it was expected would have been read before the meeting that evening, had unfortunately been lost in transmission through the post, and they were consequently unable to read it.

The Secretary called attention to a slide exhibited by Mr. Baker, just received from Herr Möller, and a very remarkable specimen of his extraordinary skill. In a square, with sides only $\frac{1}{10}$th of an inch, were 80 clear circular spaces in a dark framework of photography, and in each space a fine specimen of a diatom, with its name, and the authority for the name, plainly photographed below it. The whole series could be well seen at one glance under a $1\frac{1}{2}$-inch objective, and the names read, though very small with that power unless a B eyepiece was employed. Beck's $\frac{1}{8}$th had a considerably larger field than one of the spaces, and Powell and Lealand's immersion $\frac{1}{8}$th just took one in, and showed in one view a name as long as "*Triceratium formosum,*" the letters being beautifully sharp with that magnification.

It was stated that Herr Möller prepared slides with 100 as well as those with 80 specimens, and was about to introduce similar slides of Echinoidea, Holothuridæ, &c. With difficult objects like diatoms, the advantage of having a well-assorted series in one slide with the names attached was obviously great, and would no doubt be appreciated by all students as well as by collectors of microscopical curiosities.

Mr. Slack also said that he was asked at the last meeting if he had seen silica solution in the milky condition described by Mr. Read.* Since that meeting he had received from Mr. Read a specimen, the whole of which was milky, and the question was whether the particles could be seen. He had examined some of the fluid with various powers and under different illuminations, but had not succeeded in seeing the particles. If a drop or a thick film were put upon a glass slide and evaporated, the result was a film of the silica with the cracks in it; but if a thin smear only were put on the glass, then they got thousands of spherical particles. The deposit was probably a hydrate of silica, and he thought it possible that particles ought to be seen if the power employed was good enough. He had sent some of the solution to Dr. Anthony, and it appeared that he did see numerous aprticles with a $\frac{1}{2}$-inch objective by Ross, in a dark room with a beam of light let in through a hole in a shutter, but he was unable to see them with a higher power. This reminded him of some remarks by Professor Tyndall, who had stated that if a little mastic dissolved in alcohol was added to water in sufficient quantity to produce a sky-blue effect, the particles could not be seen by any known microscopical powers; but if a small drop of it were taken and put into a little water, then they could be seen. Why was it that they could be seen in the small quantity, and yet not in the large? He had not yet material enough to write a paper on the subject, but thought he might mention it as being one of interest.

The President believed that it was entirely a matter of molecular aggregation. He remembered that some years ago the late Professor Faraday gave him a bottle of liquid containing gold in a minute state of subdivision, so that the fluid appeared of a rose colour. He submitted it to examination with the highest powers, and illuminated it in all ways, but was not able to trace any sign of the particles. It remained so for many years, and at the present time there was a quantity of the minute particles of gold at the bottom of the bottle as a sediment; but if shaken up, they remained merely mechanically suspended in the liquid, and could be seen as molecules, which settled again to the bottom in the course of a short time. In dissolving a substance it might be that its molecules were so widely separated that they passed beyond each other's sphere of attraction, and that if placed in a small quantity of fluid they might be brought nearer, and thus within the sphere of mutual attraction. He believed another instance was furnished by the preparation of chromate of lead which was used for injections. These injections it was found could only be made with a solution of the precipitate which was freshly prepared; if it were allowed to remain a long time, the molecules became larger, and the finest capillaries could not any longer be injected with them.

* 'Monthly Microscopical Journal,' June, 1874, p. 272.

Mr. Chas. Stewart communicated to the meeting a short note upon the position of the touch-corpuscles in the human skin. His attention had been drawn to the subject by a paper written by Dr. Thin, and though he agreed generally with the writer as to their structure, he could not do so altogether as to their position. Mr. Stewart then proceeded to explain by means of drawings upon the black-board the structure of the palmar skin of the hand and the plantar skin of the foot, as distinguished from that of the other parts of the body, and showed the peculiar position in the skin of the finger of the touch-corpuscles. The results of many observations showed that they were invariably situated in those papillæ which were nearest to the furrows of the skin, and never in those nearest to the sudoriferous ducts. He did not yet see why they should be so placed, but their position there he had found to be invariable.

Mr. Stewart also called attention to some prepared sections of an ascidian (Botryllus) which he exhibited in the room. His method of killing them was to place them first in a glass of water until they opened out and were in full action, and then to plunge them immediately into strong methylated spirit. The change was so sudden that they opened their mouths in the new element, and immediately died the death of the drunkard. If weaker spirit were used, they took some of it in, but immediately closed up, and would have no more to do with it. The mounted section exhibited had been killed in this way, and was stained slightly with hematoxylin. Mr. Stewart then drew upon the black-board the section referred to, and explained the structure and action of the various parts as he proceeded. He thought that it might be of interest to some persons to know that many most beautiful forms of these creatures could be easily prepared and preserved.

On the motion of the President, votes of thanks to Mr. Stewart for his interesting communications were unanimously passed.

Dr. Matthews said he should like to hear Mr. Stewart's opinion as to the Pacinian bodies of the mesentery.

Mr. Stewart thought there was really very little resemblance, but the preparation was perhaps worth mentioning. He had found a good way was to cut out a piece of mesentery, put it into Muller's fluid for three weeks. After that dissect off one half, and place it in a little weak spirit and water; then transfer it to absolute alcohol for the purpose of hardening, and afterwards into a very weak solution of hematoxylin to stain it. On taking it out of the staining fluid it should be put to harden again in spirit, and afterwards mounted in balsam in the usual way.

Dr. Braithwaite said he should like to observe that in the first diagram drawn by Mr. Stewart (that of the section of skin of human finger), although he had every respect for Mr. Darwin, it seemed to him that those little tacti were really placed in just the very best position for the purpose for which they were intended, and that they were so placed as not to interfere with the sweat-gland appeared to him very indicative of design. He thought the meeting was very much indebted to Mr. Stewart for the very lucid manner in which he had described these very interesting bodies.

Mr. Stewart was hardly prepared to say why the corpuscles were placed as above described; they might, for instance, have been placed upon the top of the ridge and nearest to the surface which first came in contact with the objects touched. In their actual position the distance was greater from the surface of contact than it otherwise might have been. He confessed that he could not quite as yet see why they were placed in that particular position.

Mr. Slack thought that, as a mechanical question, the top of the ridge being thicker than the intermediate space, these little bodies might be really in the position of greatest impression.

Mr. Sanders asked Mr. Stewart how the ascidian was hardened.

Mr. Stewart said that at the time of killing them he put them into strong methylated spirit 80° over proof, and then afterwards put them into absolute alcohol.

Dr. Matthews said that in killing these creatures suddenly he had adopted the plan of introducing the spirit through a tube put down to the bottom of the vessel, and thus displacing the sea water.

Mr. Stewart had adopted this plan sometimes in killing zoophytes, adding the spirit very carefully drop by drop; but with the ascidians he found that any plans of this sort did not answer so well as putting them suddenly into strong spirit, and letting them get a good mouthful of it before they knew where they were.

Dr. Matthews said part of his plan was to cork the end of the funnel, and to let the spirit go in with a rush: the difference in the specific gravity of the two fluids rendered this necessary.

The President wished the Fellows of the Society a pleasant vacation and many opportunities of increasing their stock of objects; and the meeting was then adjourned to October 7th.

Donations to the Library since May 6th, 1874 :—

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Sixteenth Report of the East Kent Natural History Society for 1873	*Ditto.*
Proceedings of the Bristol Naturalists' Society. New Series. Vol. I. Part I.	*Ditto.*
Journal of the Quekett Club. No. 26	*Club.*
The President's Address, &c., of West Kent Natural History Society for 1873	*Society.*
The Canadian Journal. No. 80	*Canadian Institution.*
Bulletin de la Société Botanique de France	*Society.*

The following gentlemen were elected Fellows of the Society:—

Frederick William Hembry, Esq.
Dr. Arthur Jukes Johnson.
William James Lancaster, Esq.
Samuel Petty Leather, Esq.
Dr. William Radford.
Roland Dunn Smith, Esq.

WALTER W. REEVES,
Assist.-Secretary.

MEDICAL MICROSCOPICAL SOCIETY.

The twelfth meeting of this Society was held on Friday, March 20th, at the Royal Westminster Ophthalmic Hospital, Jabez Hogg, Esq., President, in the chair.

The minutes of the last meeting having been read and confirmed, Mr. George Giles read a paper "On Staining with Aniline Dyes for Balsam Mounting."

The author of the paper was first led to study this subject from reading the following passage in Frei's 'Technology':—

"It is very unfortunate that alcohol soon extracts the colour [of aniline-red], so that it is impossible to preserve the specimen in Canada balsam."

To obviate this inconvenience he tried a 2 per cent. solution of aniline in spirit, and then found that by staining sections that had been in spirit with this solution for three or four minutes, rinsing in spirit and placing subsequently in oil of cloves, the colour was perfectly preserved when the specimen was mounted in Canada balsam. Oil of cloves was preferable to turpentine, the latter at times precipitating the colouring matter; but should this occur, brushing with a camel's hair pencil would remove the deposit. Mr. Giles claimed three advantages for this method: 1st, Its cleanliness; 2nd, That one has the most perfect control over the depth of colour obtained by regulating the time of the subsequent washing in spirit; 3rd, That the colour is less trying to the eyes than that of carmine. Its selective power was greater than that of Frei's aqueous solution of aniline. The nerve fibres of the spinal cord, as well as the nuclei of cells, being vividly brought out.

The Secretary then read a paper by Mr. E. C. Baber—who was unavoidably absent — upon "Staining with Picro-Carminate of Ammonia."

PREPARATION OF PICRO-CARMINE.

Pure Carmine	1 grme.
Liq. Ammoniæ	4 c. centres.
Water	200 grmes.

Mix; and then add

Picric acid	5 grmes.

Agitate from time to time during two days; allow residue to settle; decant and evaporate decanted liquor at the temperature of the air; redissolve the crystals in water (strength, 2 p. c.); and filter if necessary.

In the discussion that followed,—

Dr. Matthews remarked that some tissues attract red rather than purple colours: thus, nuclei generally were more easily stained by the former. Judson's dyes he had found useful. Referred to Frei's methods of employing picro-carmine; had obtained good results from first staining in carmine, and subsequently in a solution of picric acid. He had found Stevens' writing fluid a ready and useful stain for sections.

Mr. White had found a section of epithelioma, stained with log-

wood, and then with picric acid, showed the yellow centres of the "birds' nests," described by Mr. Baber, while the surrounding parts were tinted by the hæmatoxylin. Had only used carmine and picric acid as separate solutions, but by this means had seen yellow channels of communication from one "bird's nest" to another.

Mr. Kesteven asked if aniline dyes were permanent.

Mr. Atkinson found that crystallized magenta, when first used for staining sections, became blue, and then after a time disappeared; but mounting in $\frac{1}{3}$ per cent. of corrosive sublimate prevented this.

Mr. Schäfer had given up carmine because of its too brilliant colour, and always used logwood, which he found selective in property. Thought osmic acid better than picro-carmine for nerve tissues; and remarked that Dr. Sharpey had long ago used magenta for staining the axis cylinders of nerves.

Mr. Miller had found a solution of carmine and a 4 per cent. solution of picric, and in alcohol and water especially, good for spleen and unstriped muscular fibres. He preferred carmine to logwood.

Mr. Groves, except in the case of nerve structures, preferred logwood to carmine. The double staining of logwood and gold chloride was good for nerves and nuclei, and especially, for such structures as frog's bladder.

Mr. Golding Bird mentioned Dr. Moxon's use of Stevens' writing fluid for staining nerve structures, and mentioned a fact communicated to him by Dr. Malassez of Paris, that aniline dissolved in spirit was especially good for studying ossification of cartilage; for it stained the cartilage, but not the newly-formed bone; while an aqueous solution of aniline stained the cavaliculi and not the bone substance, but was not permanent like the alcoholic solution.

The President, in proposing a vote of thanks to the authors of the papers, and which was duly accorded, remarked that more investigation was required on the subject of staining fluids, and recommended it as an object of special study, that would certainly be productive of useful results.

Mr. W. B. Kesteven then read a paper upon "Miliary Sclerosis." The subject of this paper was a form of grey degeneration occurring in the brain and spinal cord, and designated by Drs. Batty, Tuke, and Rutherford, "Miliary Sclerosis." The author showed examples of this lesion by sections and drawings. The change, he stated, is associated with a wide range of diseases of the nervous centres. He enumerated as many as twenty morbid conditions in which he had met with the so-called miliary sclerosis. The essential characters of this lesion Mr. Kesteven showed to consist in the absence, in circumscribed patches of the normal nerve tissue, and its replacement by an altered and degenerate state of the neuroglia. The spots vary in size from $\frac{1}{50}$th inch to $\frac{1}{500}$th inch in diameter. Their physical characters were described in detail, and the author then proceeded to discuss the question of how this change was connected with previous symptoms, and whether it is possible that they could be the result of mere post-mortem changes. These questions, he submitted, were as yet unanswered. Judging from the great diversity of pathological con-

ditions in which this degeneration is met with, he deemed the solution of the problem impossible with our present amount of knowledge in neuro-pathology.

Dr. Payne asked whether Mr. Kesteven had found miliary sclerosis in a spinal cord or brain, otherwise quite healthy, and discussed the question as to whether the changes described might not be the commencement of secondary degenerations of nerves, as is seen to result from inactivity of a nerve arising from any cause; or of the wasting of certain nerve fibres, that might go on to worse changes.

Mr. Schäfer took exception to the name, as giving the idea of fibrous or cecatricial tissue, whereas what had been described was rather colloid in nature; for at times it could be stained intensely. He had seen "miliary sclerosis" in the brain of a supposed healthy dog, that had been hardened in chromic acid; and from this he concluded that the alcohol used to prepare the specimens could not be the cause of the "sclerosis," as had been alleged, seeing that he had used none. As the disease followed no special tracts, he considered it could not be simply degeneration of nerve fibres.

Dr. Matthews asked whether coincident disease—as atheroma—of the vessels of the brain had been noticed.

The President had seen miliary sclerosis accompanied by calcareous change in the vessels, and in a case where death resulted from cerebral hemorrhage; also in preparations of brain made by Dr. Crisp from the lower animals, and hardened in chromic acid.

Mr. Kesteven, in reply, considered the term "sclerosis" more applicable to the cases where the disease occurs *en plâques*. There was nothing of fibrous nature in the condition he had been describing; still, Dr. Tuke had given the name originally. He did not consider the alteration colloid, though at first sight resembling it; nor had he noticed the change in connection with atheromatous vessels, though at times the bodies described were calcareous and gritty ("brain sand"). Agreed with Mr. Schäfer in not considering the condition as one of nerve fibre degeneration, and was in fact still seeking an explanation.

With a vote of thanks to Mr. Kesteven, by the President, the election of new members, and an announcement of a gift of slides to the Society's cabinet, the proceedings terminated.

The principal specimens exhibited under microscopes during the evening were in illustration of the papers read.

At the meeting of this Society, on April 18th, Jabez Hogg, Esq., President, in the chair, Dr. Greenfield read a paper upon "Diphtheria."

In this paper, which was founded upon the microscopical examination of specimens from five cases of diphtheria, which was illustrated by preparations, the author, in remarking upon the obscurity and doubt which still seemed to exist upon the origin and structure of the diphtheritic false membrane, stated his belief that this arose in part from the confusion in the nomenclature in common use, especially the fact that "croupous" and "diphtheritic" were terms used in different senses, clinically and histologically.

An examination of his cases showed in all, in the larynx and

trachea the mucous membrane and usually the deeper tissues in a state of more or less intense inflammation of ordinary character; whilst the false membrane consisted for the most part of a stratified network of a substance giving the reactions of fibrin, in the meshes of which were contained altered epithelial cells and corpuscles.

The amount of adhesion to the mucous membrane was various, but in no case did the exudation actually pass into its substance; although in some cases it appeared adherent by fibrinous bands to the papillæ.

After describing the views of Wagner and of other German pathologists, the author stated his belief that the false membrane consisted in part of a catarrhal process, with modifications in the epithelium; and in part of a true fibrinous exudation. These views were supported by the comparative examination of specimens taken from cases in various stages.

In the pharynx the inflammatory process was stated to extend much deeper than in the trachea, and to be accompanied by a more rapid destruction of tissue. The false membrane was believed to consist in a larger measure of altered cells.

The question of the occurrence and importance of fungous growth in the mucous membrane was then discussed, and the author showed specimens from the pharynx containing numbers of minute fungous spores and a delicate mycelium deeply penetrating the inflamed mucous membrane. He had not, however, been able to find a similar appearance in the larynx and trachea of the same or other cases; and he considered it therefore still an open question how far the fungus was an accidental occurrence and what was its relation to the disease.

The President, after proposing a vote of thanks to the author of the paper, stated his belief that fungous growths might be always found in the mucous membranes in certain low states of health, and considered a fungus in diphtheria an accidental rather than an essential occurrence. He could not agree with Dr. Oscar Giacchi, who held that the disease was owing to the presence of a fungus. He had examined more than one case of diphtheritic conjunctivitis, in which disease the exudation forms very rapidly, but had never found any fungus. The position of a vegetable parasite upon the body had much to do with its influence upon the disease it accompanied, or of which it was the cause. Hence some importance might be attached to the specimen shown, where the fungus was deep in the inflamed mucous membrane.

Dr. Bruce remarked that croup is generally defined as owing to a false membrane, on the removal of which healthy mucous membrane is left; this, however, the paper would disprove, since Dr. Greenfield had shown that not only the mucous and submucous tissues were at times reached in croup, but that even the tracheal rings might be in part destroyed. He had also noticed the small cavities or vacuoles described in the false membranes, and thought them owing to the exudation from the ducts of mucous glands; indeed, these spaces at times were filled with exudation cells. The mucous epithelium is not necessarily destroyed by the false membrane; it may sometimes be seen covered by the latter. Exudation of fibrin would fully account

for the false membrane upon the mucous membrane, without interfering with the epithelium covering the latter, through which wandering cells might easily pass: and a precedent for fibrinous exudation on a mucous surface might be found in croupous pneumonia.

Mr. Needham thought that as pus could come from a serous, fibrin might from a mucous membrane.

Dr. Coupland thought the different layers in the false membrane showed a mixed origin; thus the surface of it was more coarsely fibrillated than the deeper parts, which were much finer, as though there had been first a catarrh, destroying the epithelium, and then fibrinous exudation last of all.

Mr. Stowers asked for a verification of the observation made, that the histological appearances in the angina form of scarlatina and in a blistered surface were those of diphtheria.

Mr. Miller referred to Rindfleisch's remark that the exudation in pharyngeal affections was more cellular and less fibrillated than in laryngeal; as well as to the existence of apertures in the basement membrane of the affected parts.

The President thought the non-homogeneity of the false membrane might be explained by the different ages of its component parts, and suggested that the fungus found in throat diphtheria might owe its presence to the open-mouthed mode of respiration in diphtheritic patients. In the two cases of diphtheritic conjunctivitis already mentioned, the eyes had been kept bandaged, and, as stated, no fungus had been found.

Dr. Greenfield, in reply, quoted German authority for the constant presence of fungus in diphtheria; and since fungi in the kidney had also been described, they might serve to explain the renal complication so constantly present. The only way to get at a life history of a false membrane was to examine in the same subject the patches in all stages of growth. He had done this, but had only found at first a catarrhal state, and later on pus-globules and fibrillation on the deeper surface of the membrane. The fibrinous exudation in pneumonia was no precedent for the same process in diphtheria, since the air-cells might be proved, and were by some thought to be, part of the lymphatic system. Epithelium in place would not allow fibrin to exude; but once destroy the former, and then exudation was easy. Two theories existed with regard to the part played by fungi in diphtheria; one, that they were its cause, the other only the cause of the rapid disintegration of the membrane: it was a subject still *sub judice.*

Numerous specimens in illustration of Dr. Greenfield's paper were exhibited, as well as others of new growths, and of the glandular stomach of the crow.

Friday, May 15, 1874.—Jabez Hogg, Esq., President, in the chair.

Molluscum fibrosum (*? Cheloid*).—Dr. Pritchard mentioned the case of a negro, who for twenty years had been subject to a growth, originating behind one ear, and gradually extending over nearly all the body. After death, a portion of the skin, with growth, was forwarded to him (from America) as illustrating "Cheloid simulating molluscum

fibrosum." Microscopically the cutis vera was found hypertrophied; here and there masses of cells between the fibres of the areolar tissue. Epidermis much thickened — hair-follicles normal. Papillæ had grown irregularly and sideways, and not vertically as normal. He considered it simply a case of molluscum fibrosum, not of Cheloid. Engravings of the patient, with specimens of the disease, were exhibited. The President inquired if any fungus had been noticed; it was by some believed to exist in molluscum. Mr. Needham thought this condition of the papillæ normal in the negro. Dr. Pritchard, in reply, had observed no fungus.

Perivascular Spaces in the Brain.—Mr. Kesteven read a paper on this subject, illustrated by drawings and specimens. These spaces had been considered normal structures, intended to relieve intracranial blood pressure; but Mr. Kesteven had never seen them in a really healthy brain: had often noticed them associated with chronic cerebral mischief, and hence concluded they were owing to absorption of brain substance by the irregular circulation that goes on in chronic disease: the vessels being at one time full, at another nearly empty. Though the mode of preparation in chromic acid might render these spaces more evident by the shrinking of the blood-vessel, he did not think it sufficient to account entirely for them. He could find in the perivascular spaces no resemblance to normal lymphatic structure, while Dr. Batty Tuke had now abandoned the idea that they denoted a healthy condition of brain. Dr. Pritchard considered them entirely owing to the mode of preparation in chromic acid; he had never found them in sections made by freezing the brain. Mr. Needham argued their belonging to the lymphatic system, though not strictly speaking "lymphatics." The President explained them in some cases by the giving way of the capillaries around which they were found: he had seen the brain substance stained with hœmatin in their vicinity; hence an explanation, perhaps, for some of the anomalous convulsions of childhood: thought proof was wanting of their connection with the lymphatic system. Mr. Tirrard asked if, in injected brains, these spaces were seen, or were obliterated by the distension of the vessel. In reply, Mr. Golding Bird stated that he had never seen them in injected specimens. Mr. Groves asked if Mr. Kesteven had ever examined the spaces by staining with nitrate of silver. Mr. Kesteven, quoting Dr. Batty Tuke, stated that the spaces had been found in the lower animals (*e.g.* cats) after strangulation; and that, though the vessels thus remained full, a space could be seen beyond. He had never seen anything to warrant the supposition that they were owing to hemorrhage. He knew of no anatomist having traced these spaces into lymphatics; they had been injected by His by the puncture method.

Multiple Cystic Tumour of Heart.—A specimen of this, exhibited by Mr. Needham, seemed to show, from the excess of epithelium in the mammary tubules, and the epithelial infiltration of surrounding parts, at once a cystic, an adenomatous, and cancerous nature. Mr. Needham founded his idea of cancer on the arrangement, and not on the intrinsic form of the cells composing it.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

April 23rd.—Microscopical Meeting. Mr. Haselwood, President, in the chair.

The receipt of eleven micro-photographs, by Dr. Hallifax, for the Society's album, was announced, and a vote of thanks given to Dr. Hallifax for the same.

Mr. Wonfor on "Plant Crystals."

In most of the manuals on botany or the microscope, certain crystalline bodies found within the cells of plants were designated by the name *Raphides*, or needle-shaped bodies, a term inapplicable to some, because they were not needle-shaped, and on the whole misleading, because in the lists of plants generally given as containing them, it would seem as though their appearance was an accidental circumstance in the economy of the plant, instead of a constant quantity, not confined to one period of the plant's growth, but found in all stages of its existence.

The first to reduce to something like order and to indicate the value of plant crystals in determining the differences between plants of otherwise closely-allied families was Professor G. Gulliver, by whom they had been arranged into the three groups—*Raphides*, *Sphæraphides* and *Crystal Prisms*.

Raphides were transparent colourless crystals, needle-shaped, and tapering to a fine point at each end, loosely connected in bundles of twenty or more in oval or oblong cells of slightly larger dimensions than themselves. The slightest pressure on the tissues of a portion of a plant, when under examination, caused them to escape from their cells. Examples could easily be obtained either by making thin sections or simply tearing with needles a portion of the tissues of any member of the balsams, woodruffs, evening primrose, arum, orchis, and some of the iris family.

Sphæraphides were more or less rounded, often spherical bodies, made up of white or opaque crystals or crystalline matter. In some the ends of the component crystals projected and gave them a star-shaped appearance. They were much smaller than their cells, and, in some cases, were thickly studded throughout the cellular tissue. This was the case with many of the cactus family, some of which, when aged, had their tissues so filled with them, as to render the plants very brittle. It was mentioned that plants of *C. senilis*, reported over a thousand years old, when sent to Kew Gardens, had to be packed in cotton as carefully as if they were delicate glass or jewellery. The fruit of the prickly pear was full of *sphæraphides*, examples of which were easily seen in the crane's-bills, elm, beet, spinach, or violets.

Crystal prisms were found either singly, or two, three, or, at most, four together, in combination in the same cell. While under a low power *raphides* did not present angles or faces, crystal prisms presented both; instead, too, of tapering to a point at both ends, they were wedge-shaped or angular. Some were three or four sided, while others were octohedrons. They were larger than *raphides*, and were not, as a rule, easily separated from the tissues in which they were

seated. Examples might be found in the green-pea shell, the garlic, green fig stem, gladiolus, &c., and very abundantly in the soap tree of South America, used in Peru as a substitute for soap in the cleansing wool or hair, or in washing.

Chemically, plant crystals were chiefly composed of oxalates of lime and magnesia, or phosphate of lime.

Professor Gulliver's researches showed that so persistent were *raphides* in certain families of plants, and so absent in others, that it was possible to differentiate, at all stages of their growth, between plants otherwise apparently allied. Thus *Onagraceæ* and *Galliaceæ* abounded in *raphides*, while none of their near neighbours contained them. So likewise the red berries of black bryony and cuckoo-pint could be distinguished from those of red bryony and guelder-rose, by the presence in the first two and the absence in the last two of *raphides*.

To the botanical student characters such as those indicated were of very great value, and to the microscopist a wide field of research was opened, for not only would he find plants containing one or other of the plant crystals described, but great differences in their shape and size; many of them, too, under polarized light were very beautiful; while a lesson was to be learnt by all, that they were not accidents of decay or disease, but part of the economy of life in the plants in which they were found.

After a discussion, in which Drs. Hallifax, Corfe, the President, Mr. Glaisyer, and others took part, Mr. Henry Lee, who had been recently elected an honorary member, presented for distribution specimens of the skin of the rough-hound, and explained the mode of mounting and also of separating the spine-like scales, by dissolving away the animal matter by means of *liquor potassæ;* by this method the spines with their socket attachments were well shown. He hoped in time to bring down from the aquarium skins of all the dog-fishes, so that the members might each have a perfect series in their cabinets.

A vote of thanks was given to Mr. Lee.

Reading Microscopical Society.*

March 3.—Mr. Tatem exhibited an ant-like insect, from Ceylon, belonging to the order *Heterogyna*, sub-order *Mutilla*, insects of solitary habits, each species being composed of winged males and apterous females; the latter always armed with a powerful sting. The insertion of the antennæ near the mouth would indicate the affinity of the specimen with the genera *Dorylus* and *Labidus*, Indian, African, and South American insects, found in dry sandy districts, running with great speed and actively predaceous. The large eyes, long legs, and stout anterior tarsi, with claws developed into powerful chelæ, show eminent fitness for the pursuit and apprehension of living insect prey, so far agreeing with the genera referred to. There are, however, some points

* Report furnished by Mr. B. J. Austin.

of divergence which render it questionable whether the specimen can be correctly referred to either; *e. g.* the head is not small, nor is the abdomen cylindric.

The specimen is to be submitted to competent authority to determine whether it may not be generically or specifically new.

Captain Lang exhibited an arranged slide of *Stauroneis acuta* in filaments and both aspects, from a gathering near Warwick (sent by Mr. Staunton); also diatoms from a gathering by Captain Perry, on the west coast of South America, the most noteworthy being what seemed a perfectly new species of *Auliscus*, and a group of *Aulacodiscus formosus*; one being evidently a newly-formed inner valve of a dividing frustule. He also showed 6 and 8-rayed specimens of *Aulacodiscus Kittonii.*

Margate Microscopical Club.*

The members of the Margate Microscopical Club gave their first public soirée at the Royal Assembly Rooms on Thursday, March 26th.

Owing to illness Prof. Gulliver was unable to attend and give his lecture "On Raphides and other Plant Crystals." However, Col. Cox, of the East Kent Natural History Society, discoursed on "Agates," and explained the gathering and polishing of his beautiful collection of British agates, many of which, although very valuable, were found on Hastings and Dover beaches, being principally derived from the lower chalk formation.

Mr. Henry Lee detailed the observations made at the Brighton Aquarium upon the "Ova of the Dog-Fish," including the habits of the fish, the mode of deposition of eggs, duration of hatching, and the nature of the studies now being undertaken by Prof. Huxley and Dr. Kitchen Parker on the nervous development and skull formation.

The speciality of the evening was the display of living marine organisms, which were very successfully exhibited, and, whilst they charmed and fascinated the eye, nothing could exceed the interest taken by all the large company present in watching the beautiful movements and ciliary action of life.

It is by no means an easy task to prepare and exhibit to advantage a living object for a soirée, but the following were successfully displayed:—Membranipora, Bowerbankia, Pedecellina, barnacles, numerous eggs of annelids, doris spawn, young whelks and periwinkles on the point of hatching, fish and crab spawn, entomostraca, small crustaceans, &c.

The Margate Club has only been established three years, and numbers forty members.

Its principal aim is to study the marine life of the Thanet coast, and, whilst it entertains its fellow-residents with successful gatherings like the present, it also trains men to observe and be acquainted with the various forms of marine life, so that, if the proposed aquarium, designed upon the Brighton scale (although less ornate in appearance), and by the same engineers, be successfully established, few clubs can be more favourably situated for valuable and scientific observation.

* Report furnished by Mr. F. B. Kyngdon, Hon. Sec.

QUEKETT MICROSCOPICAL CLUB.

April 24th.—Ordinary Meeting. Dr. Robert Braithwaite, F.L.S., President, in the chair.

After the formal business was concluded, a paper by Mr. G. J. Burch, "How to make Thin Covering-glass," was read.

The mode adopted was to seal up the end of a glass tube of about $\frac{1}{4}$-inch bore with the blowpipe, soften it in the flame, then remove it, and blow through it as strongly as possible, so as to form a large and very thin bubble; this was to be broken and the pieces cut to shape with a writing diamond. If required flat, a piece could be placed on a perfectly flat piece of platinum foil, and depressed for a moment into the Bunsen flame. In this way Mr. Burch produced glass $\frac{1}{2500}$th of an inch in thickness.

Mr. Ingpen read a paper "On a False-light Excluder for Microscopical Objectives." This consisted of a cap, having a perforation a little larger than the field of view of the objective; when this was slipped on close up to the front lens, it diminished the angle of aperture; but when brought into contact with the covering glass, it allowed the full angle to be used; while in either case no rays but "image-forming rays" were admitted, and the milkiness caused by stray light was completely got rid of. This method occurred to Mr. Ingpen upon reading Mr. Wenham's letter in the March number of the 'Monthly Microscopical Journal,' describing his mode of measuring the *true* angle of a Tolles' $\frac{1}{8}$th objective.

A paper by Mr. James Fullagar, "On the Development of *Hydra vulgaris* from Ova," was read, and illustrated with several beautifully executed drawings of *Hydra* in its various stages. *Hydra vulgaris* differed in several respects from *Hydra viridis;* the egg being larger, and studded with what appeared to be short spines, and surrounded with a gelatinous envelope, which it retained to the time of hatching. The development of a single ovum was traced from the time of its extrusion. After about fifty-five days a protuberance appeared, a slight rupture was seen in the shell, and a portion of the *Hydra* slowly emerged. In about two hours rudiments of tentacles appeared as rounded lumps, and in twelve hours the *Hydra* was fully developed, with seven tentacles, being however still attached to the inside of the shell by the suctorial disk at the posterior end of the body. After a time, varying from twelve to sixty hours, the *Hydra* finally quitted the egg, and fixed itself to the weed or the side of the aquarium. The growth of the *Hydra* was very slow, and it could not be observed to feed. After a month, some small entomostraca were put into the cell, which were seized, but not absorbed; these, however, died from the effects of the stinging power of the tentacles. The ova were not easily found, after extrusion, their gelatinous envelope speedily becoming covered with extraneous particles. After the extrusion of the ovum, the parent *Hydra* gradually diminished, and in about twenty-one days the whole body dissolved. The sperm cells continued to discharge spermatozoa into the water for some days after the separation of the ovum. The ovisac and sperm cells were generally found on the

same *Hydra*, but sometimes there were sperm cells only, when the whole body was seen to be studded with them. The diameter of the ovum was about $\frac{1}{50}$th of an inch, that of *Hydra viridis* about $\frac{1}{66}$th. *Hydra vulgaris* is reproduced from ova in the autumn, *Hydra viridis* in the spring.

Mr. Ingpen exhibited and described an achromatic bull's-eye condenser, formed out of the objectives of a binocular opera-glass. One of the lenses was reversed in its cell, and the two screwed into the opposite ends of a short piece of tube, so that the flat side of one nearly touched the convex side of the other. The light thrown by this condenser was very pure, and those who possessed a binocular opera or field-glass could construct one at a small expense, while the lenses were not spoilt for their original purpose.

The President announced that at the next meeting he would read the fifth of his series of papers "On the Histology of Plants."

The meeting concluded with the usual conversazione, at which several interesting objects were exhibited.

May 22.—Dr. Robert Braithwaite, F.L.S., President, in the chair.

Twenty-three members were duly elected.

The death was announced of Mr. T. W. Burr, F.R.A.S., F.R.M.S., an old and valued member and a Vice-President of the club.

Dr. George Hoggan read a paper "On a New Section-cutting Machine." This instrument differed in many respects from any of those in ordinary use, and was designed by him for cutting both hard and soft substances. Two of these instruments were exhibited, and minutely described. A stout plate of brass, sliding in a double dovetailed groove, was moved backwards and forwards by a micrometer screw. Upon this plate the substance to be cut, if hard, was fixed by one or more clamps; if soft, it was inserted in a cube of carrot, cut to fit a brass trough, which could be similarly clamped to the plate or "table," so as to move with it. Hard substances were cut with a very fine-toothed spring saw, which was guided between strong supports, so as to be capable of cutting a series of perfectly parallel sections. In one of the instruments shown, there were three of such sections from a tooth, cut very nearly through, but still attached to the rest, from which many more sections could be made. These sections were ready for mounting, the saw marks being scarcely, if at all visible. Stress was laid upon using the saw with the teeth reversed, so as to cut while pulling, and not while pushing it. Soft substances, after being imbedded in the cube of carrot, and wedged up, if required, with pieces of elder-pith, were fixed to the plate by means of the trough, and the knife or razor was guided by two parallel bars of steel, instead of sliding over a plate, by which method the edge was less liable to injury, and sections of large masses of unequal consistence could be cut with great facility, from the possibility of making a number of short cuts, instead of single sweeps only. The blade of the knife was straight, the under side flat, and the upper side deeply hollowed, so as to contain enough fluid to float off the section as soon as it was cut. The machine was rather complicated, but this was necessary, from the desire that success should depend as little as possible upon the skill of the operator.

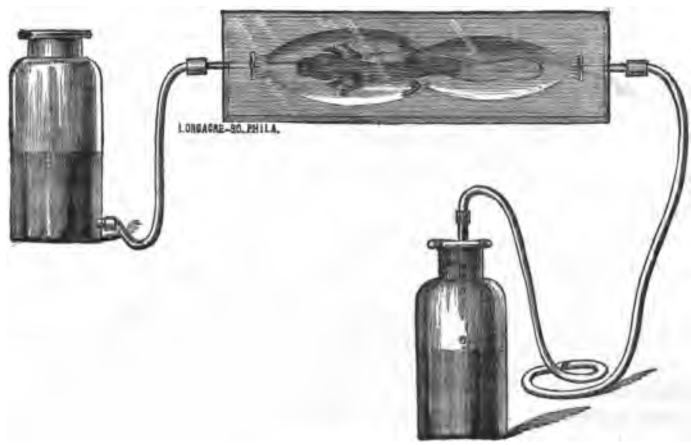
The thanks of the meeting were unanimously voted to Dr. Hoggan, and a short discussion followed. A number of beautiful specimens, both of hard and soft substances, cut by the machine, were exhibited, and much interest was shown in them.

A paper by Dr. Braithwaite, "On the Histology of Plants," being the fifth of a series written for the club, was taken as read; and it was announced that the demonstration of it, by the exhibition of a number of microscopical preparations of pith, bark, cuticle, &c., would take place at the next conversational meeting, on the 12th of June.

Announcements of meetings, excursions, &c., were made, and the meeting concluded with the usual conversazione.

Microscopical Section of the Academy of Natural Sciences of Philadelphia.

January 5, 1874.—Director W. S. W. Ruschenberger, M.D., in the chair.—Mr. Holman's "siphon slide" for the microscope was exhibited in operation by Dr. Joseph G. Richardson, who remarked that the apparatus was composed essentially of a strip of plate glass, of the ordinary length and width (namely, three inches long by one inch wide), but double the usual thickness, in the upper surface of which had been ground a shallow groove, elliptical in both its transverse and



longitudinal section, and deeper toward one extremity. The excavation was so arranged as to receive a small fish, tadpole, or triton, and retain it without, on the one hand, injury from undue pressure, but without, on the other, permitting any troublesome gymnastics beneath the thin glass cover, which, when applied, formed the ceiling of the cell. The great improvement of this slide consisted, however, in the imbedding of a small metallic tube (communicating with each extremity of the groove), in either end of the slide, and the adaptation to these two tubes of pieces of slender caoutchouc pipe, about eighteen inches in length, one of these being intended for the entrance and the other for the exit of any fluid, *cold* or *hot*, which it might be desirable to employ.

For examination of larger reptiles, and for demonstrations with the gas microscope, a slide four inches long, with two oval concavities, and a narrow groove more deeply cut for the body of the creature, as shown in the figure, has been devised. With such an apparatus, through which a current of ice-water can be passed, the injurious heating effect which ordinarily attends the use of calcium or electric light to illuminate living specimens is entirely counteracted.

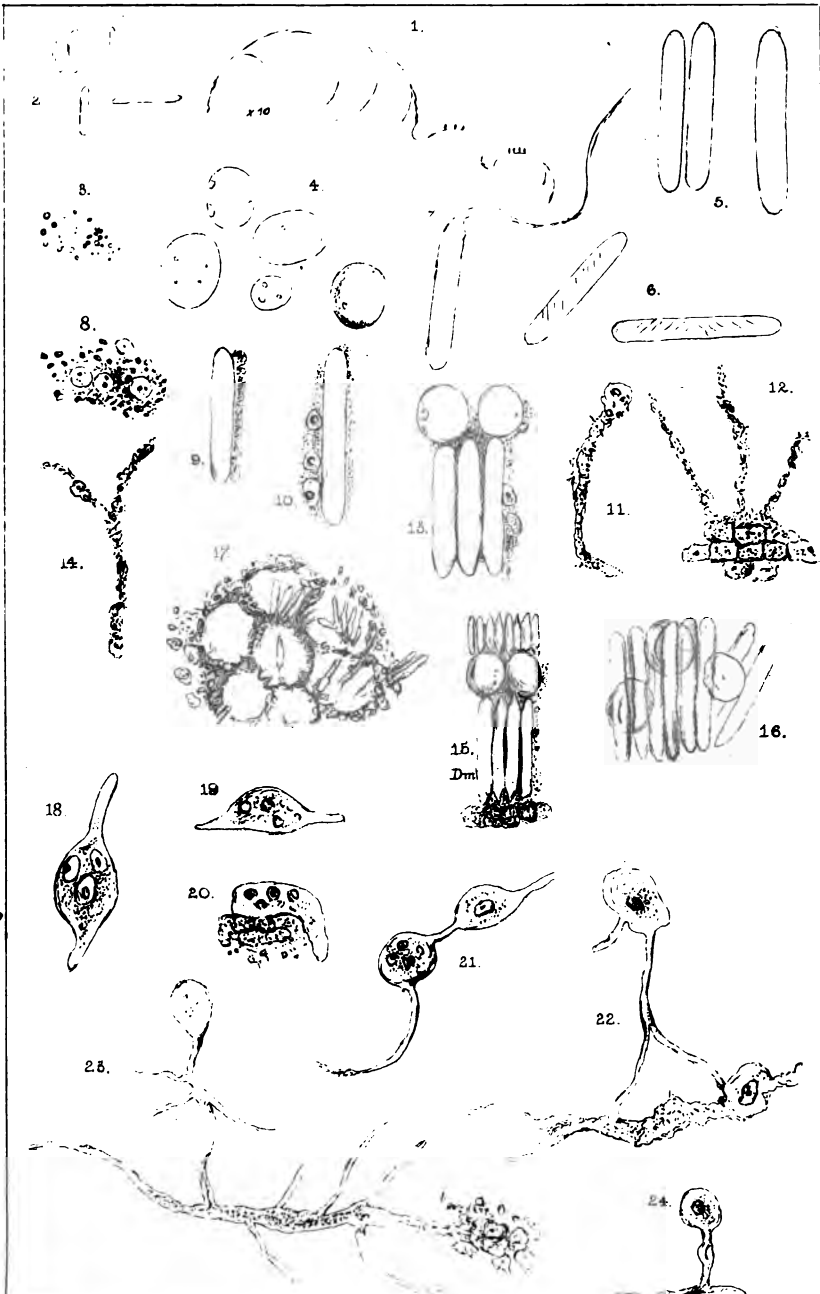
When in use it is only necessary to place the animal (in the case before us a little triton) with some water in the groove of the slide, cover him with a sheet of thin glass, immerse the end of one of the caoutchouc tubes in a jar of water, and then, applying the mouth to the extremity of the other rubber pipe, make sufficient suction to set up a flow of the liquid through the apparatus. The stream of fluid (of course bathing the animal in the cell during its passage) can readily be kept up as in any other siphon for hours or days, and its rapidity exactly regulated by graduated pressure upon the entrance pipe, so that in this way a triton may be examined continuously (as stated by Dr. J. Gibbons Hunt, who had kindly furnished and prepared the slide and specimen) for a whole week without material injury.

Among the great advantages of this very ingenious contrivance may be enumerated,—first, its security—the animal being prevented from escaping, and the joints of the apparatus being kept tightly closed by the pressure of the atmosphere; second, its portability,—the whole preparation, for example, one for showing the circulation of the blood, being made at home,—as was done in this instance,—carried to a lecture-room in the pocket, and exhibited to an audience hours afterwards; and third, its convenience,—this arrangement permitting the removal of the slide at any time from the microscope stage, to make way for other experiments, and its instant readjustment when desired.

Dr. Richardson invited the attention of members to the remarkably clear and distinct view of the circulation displayed by the aid of this apparatus in the caudal extremity of a triton, beneath one of the microscopes upon the table, and pointed out as especially worthy of note the marked prominence of nuclei in epithelial cells covering a portion of the tail, where blood-stasis had occurred, in consequence of a minute puncture, purposely made before incarcerating the reptile; suggesting that this change was doubtless the visible exponent of that pathological alteration of the circumjacent cellular elements, which constitutes such an important, although as yet but imperfectly understood, factor in the inflammatory process.

The Louisville Microscopical Society.

The Louisville Microscopical Society was organized on Thursday, January 15, 1874. The following are the officers for the ensuing year:—*President*, Prof. J. Lawence Smith; *Vice-Presidents*, Noble Butler, Dr. C. F. Carpenter; *Treasurer*, C. J. F. Allen; *Secretary*, John Williamson; *Corresponding Secretary*, Dr. E. S. Crosier; *Executive Committee*, R. C. Gwathmey, Dr. E. R. Palmer, Dr. James Knapp, W. F. Beach, D. T. E. Jenkins.



M.D. ad nat. del. H. Wesley lith. W. West & Co. imp.

Nervous system in Actinia.

THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1874.

I.—*The Presence of Balbiani's Nucleus in the Ovum of Osseous Fishes.* By Dr. Van Bambeke.

We know that according to M. Balbiani the ovule is not so simple a structure as is generally believed;* by a series of researches extended over nearly every class of animals the distinguished French embryologist is assured that besides the germinal vesicle considered as the nucleus of the egg-cell, one finds a second nucleus whose function "consists essentially in bringing about the separation of the elements, till then indifferent, of the protoplasm of the young ovule into a germinal part, and a nutritive part, grouping around the one the materials destined to form the plastic part or the germ whence is later formed the embryo, while the other or simply nutritive material remains around the germinal vesicle."† Hence the name *vésicule embryogène* given by M. Milne-Edwards to Balbiani's vesicle.

We know then generally the great importance from the point of view of embryology of Balbiani's discovery. But, as he says himself in his last work, "his conclusions have been combated by various authors." And in a very recent memoir upon the egg and its development in osseous fishes, published by one of the first embryologists in Germany—Herr W. His—there is no mention whatever of Balbiani's vesicle.‡ This it is which leads me to communicate the present paper to the Society of Medicine at Gand.

In studying the ovarian egg of osseous fishes I am certain that one finds in ovules of a certain age, besides the germinal vesicle, another nuclear mass, viz. *le noyau de Balbiani*.§ Without stopping for a

* "On the Constitution of the Germ in the Animal Ovum before Fecundation" ('Comptes Rendus,' 1864, t. lviii., pp. 584–588, 621–625). A short *exposé* of Balbiani's mode of view, accompanied by three figures (after the author), has been inserted in the form of a note by Dr. Ranvier in his translation of Frey's 'Treatise on Histology,' p. 103.

† Balbiani, "Mémoire sur le Développement des Aranéides" ('Annales des Sciences Naturelles,' 5e Série, tom. xviii., p. 33).

‡ Wilhelm His, 'Untersuchungen über das ei und die Eientwickelung bie Knocken-fischen,' mit 4 Tafeln, Leipzig, 1873. We think we recognize Balbiani's vesicle in the egg of the Barbel, Fig. 1*d* of Plate II. of His' essay.

§ I have rarely found the vesicular form, but generally that of a more or less granular mass. For this reason, and not to prejudge the function of the organ, I have replaced here the expressions *vésicule de Balbiani* and *vésicule embryogène* by that of *noyau de Balbiani*.

moment to speak of the intimate structure of this *spot*, and being still reserved on its genesis and function, the following is what I have found:—

(1) Generally Balbiani's nucleus is not readily distinguishable in ova examined in the fresh state, or in indifferent liquids. But it soon appears under the influence of certain reagents, such as chloride of gold, picro-carmine, and, above all, acetic acid.

(2) It exists in even the smallest ovules.

(3) It is always eccentric as regards the germinal vesicle, and it is generally very close to the periphery of the egg.

(4) Its volume, which is generally inferior to that of the germinal vesicle, increases with the age of the ovule.

(5) It disappears with the maturity of the ovum, consequently its disappearance precedes that of the germinal vesicle. Nevertheless, I admit this last proposition provisionally and with some doubt.

GAND.

II.—*Observations on the Tolles' $\frac{1}{8}$th.*

By R. B. TOLLES, Boston, U.S.A.

THIS paper is written as a supplement to what was sent the Journal from Florida last month. I offer items of proof and illustration not available there, the record of the $\frac{1}{8}$-inch traversed by Mr. Wenham in the March issue of the 'M. M. Journal' being in Boston. Of course I desired to speak accurately from the record, rather than trust to recollection in any important particular.

First, I will speak of the proof I have to offer of the claimed balsam angle beyond 82° as pertaining to the $\frac{1}{8}$-inch named.

With a view to doing what I could not wait to do on the eve of my departure South, I have had a $\frac{1}{8}$-inch made closely to the formula of the $\frac{1}{8}$-inch concerned. My plan for proof of outside angle was to cut off or intercept all the (cone of) light entering the objective—*dry*, by means of a central stop placed upon the posterior surface of the back system. A stop of 0·39″ diameter accomplished this, while the clear aperture of the back system being 0·44″ a ring of clear aperture remained, of appreciable breadth, beyond the opaque circular stop. And now, observe,—with air as the front external medium *it is a fact* no light came through the objective to the eye at the eye-piece.

With no matter what obliquity, the light could not be made to pass the stop to the eye. With balsamed alike with dry object darkness was the effect. To test the question of admissibility of

more than 82° of pencil with a balsam-mounted object, something more has to be done, as obviously only 180° could enter the front surface of the *slide.*

To give access of more pencil to the object I used the semi-cylinder. This simple piece of apparatus applied to the front surface of the slide *dry*, should, *if* the objective have 180° of air aperture, show 81° (+) of pencil on the cylindrical surface thereof. Without the stop on the back such was the fact, viz. 81° at the cylindrical surface, and that being the internal angle of 180° of air angle (or infinitely near that) at the plane dry surface of the slide, as also at the plane front surface of the objective, when dry. Replacing the "stop" on the back surface, again no light passed to the eye. A perfect eclipse existed. And next, to test the question of more than equivalent pencil for 180° external angle when the object is immersed in balsam or other preservative medium,—air was replaced with water above and below the slide containing the object mounted in balsam. And now, darkness no longer, but a flood of light illuminating the object with good definition of its features.

The object actually used was a fine Rhomboides, of which the cross-lines were sharply defined, using an eye-piece about one inch equivalent focus. Image-forming rays, evidently and certainly *outside* of the interior or balsam pencil of 180° *external*, and practically utilizing decidedly more than that. The balsam pencil obtained was more than 98°. I might rest here and leave Mr. Wenham to appropriate reflections over (so it appears to me) his very hasty edict against my innovating $\frac{1}{8}$-inch objective.

But, as it seems rather necessary, I will state, as explanatory, that I used $\frac{1}{210}''$ covering glass (rather less), and the systems were but slightly closed from the extreme open-point.

Very different indeed from the forced case he made of it. In this matter a singular precipitating tendency to be either at one extreme or the other extreme, fully "open" or fully closed, seems, with my critic, a dire necessity when he assumes the judicial. *Mr. Wenham had not adjusted for maximum angle at all,* nor tried to. Just and merely "closed" to avoid "after quibbles." This is verily amazing. How about *prior* "quibbles," Mr. Wenham? Who *is* at fault? But to the point again. The suggestion that 0·013 focal *distance* as in the figure making necessary limit of angle to 118° falls to the ground in view of the recited experiments. As already stated, the objective (in the experiments described) was adjusted nearly entirely open. This of itself annihilated the 0·013″ distance, sending his figure to the realms of fancy. Not "abstruse," this teacher encouragingly says. It is less than that, sir, and worse,—it is *spurious.* There is no such thing in the case. With a dry object mounted on the *cover* there is *no* distance involved (very possibly indeed, and as will occur in every such dry-mount). As there is

F 2

no distance, you are invited to "plain measurement" of whatever triangle in the case. Deny if you can.

Furthermore, anyone having under such covering glass a dry object in view with this (London) $\frac{1}{8}$-inch, when it is adjusted to good definition of the object by *extremest* obliquity of the radiant-light, and using this light directly, i.e. without intervening condenser of any sort,—then, it will be found, that the *extremest* obliquity that *can be* made incident upon the bottom surface of the *slide* will enter the objective, and it will be manifest enough that the only limit of obliquity of incidence of illuminating pencil is parallelism with the surface of the slide.

Prove (do not fail to do that) that the most oblique rays actually traverse the object and constitute an image of it at the eye-piece. How?—by simply passing a card, or like thin-edged body, upward toward the slide until the very thinnest wedge of light passes between the edge of it and the slide, or the stage, as the case may be. (Plainly for *this* purpose a slide-holder *below* the stage is necessary to reach the extremest angle.)

On my way South last March, the Journal for that month came into my hands, and I read Mr. Wenham's article with no little surprise, and I determined that on my return I would immediately subject the $\frac{1}{8}$-inch to the "stop" proof, which I knew right well would be conclusive. Meantime I could only send the illustration and seemingly *necessary instruction* how to make maximum angle at open point, while diminishing it with closure of the systems. Not *new* in my practice by any means.

And in this connection I will gladly assure Mr. Wenham that he is not being "hoist of his own petard." Not so bad as that. The bitter reflection may be dismissed. He has had nothing to do with the machinery of the *hoisting*, if that operation is going on; has not furnished ammunition, nor plan, in any particular. I have not even read his "writings" referred to.

I invite Mr. Wenham's plainest comments, without considerations of any sort other than to settle the point at issue, viz. practical objective angle over 82° for objects mounted in balsam or other preservative media.

Business, business relations, "income," absence, or absent health, need not stand in the way or affect the discussion.

Since writing the above, I have noted more particularly Mr. Wenham's remarks about "getting no further than the front lens" with a diagram.

Now, in the paper sent you last month, the back and middle systems *not given* are assumed to be of angular aperture = 60°. I did not mention, what is true, viz. the "Museum" $\frac{1}{10}$-inch to which Dr. Woodward ascribes 87° hard-balsam angle, has, inner systems, back and middle, of $62\frac{1}{2}$° angle. Now this is good as a

diagram for the argument. It is a matter of *fact*. Focus, clear $= 0 \cdot 10''$, angle $60° +$. Obviously the outside rays of this cone can be traced from behind the front to its inevitable focus in front. But, if considered requisite, a back and middle appropriate shall be given.

P.S.—I have rigged two semicircles divided for degrees to the bottom of my zinc stage, with a shutter between, movable through 180° of arc. The object-slide is also held and moved against the lower surface of the stage. The very least and last light admitted by the shutter to impinge on the face of the slide gives an image of the object,—with the choked, and smothered, and scouted $\frac{1}{8}$-inch objective.

[It appears to us that a simple question is now involved in a needless mass of phraseology. Mr. Tolles objects to the measurement described by Mr. Wenham having been taken at the closed point, always considered that of greatest aperture. If this is a mistake it may surely be tested. We have no doubt that Mr. Crisp will permit the apertures of the $\frac{1}{8}$th to be measured at any other position of the adjusting collar that Mr. Tolles may suggest.—Ed. 'M. M. J.']

III.—*On the Nervous System of Actinia.*

By Professor P. Martin Duncan, M.B. Lond., F.R.S., &c.

Plates LXIX. and LXX.

I. *A Notice of the Investigations of Homard, Haime, Schneider, and Rötteken, and others on the subject.*

MM. Milne-Edwards and Jules Haime* wrote as follows in 1857 concerning the nervous attributes of the group of Cœlenterata called Zoantharia:—"They (les Coralliaires) enjoy a highly deve-

---

DESCRIPTION OF THE PLATES.

Plate LXIX.

Fig. 1, which is an outline of a chromatophore, with two small ones close to it, is magnified 10 diameters; all the rest are drawn from nature under the magnifying power of a $\frac{1}{16}$-inch immersion lens and a medium eye-piece.
" 2.—Bacilli.
" 3.—Granular and cellular protoplasm between bacilli.
" 4.—Large refractile cells. Haimean bodies.
" 5.—Type α of a Rötteken body.
" 6.— " β " "
" 7.— " γ " " with a thread.
" 8.—Granular and cellular tissue between the Haimean bodies.
" 9.—Same kind of tissue in contact with a Rötteken body.
" 10.—Some cells with refractile nuclei in the tissue.
" 11.—Portion of tissue from amongst the Rötteken bodies.
" 14.—The same, with a forked end.

[Fig. 12.

---

* 'Hist. Nat. des Coralliaires,' vol. i., p. 11.

loped sensibility; not only do they contract forcibly upon the slightest touch, but they are, moreover, not insensible to the influence of light. Nevertheless, neither a nervous system nor organs of special sense have been discovered in them. It is true that Spix described and figured ganglions and nervous cords in the pedal disk of Actiniæ; but the observations of this naturalist, so far as the polypes are concerned, are not entitled to the least confidence.

"Some naturalists have supposed that the 'bourses calicinales' of the Actiniæ are eyes, and M. Huschke believes that certain capsules in the trunk of *Veretilla*, which contain calcareous bodies, are the organs of hearing. But these hypotheses do not rest upon any proved facts."

In 1864 Huxley noticed that, with regard to the Cœlenterata, "a nervous system has at present been clearly made out only in the Ctenophora."*

Homard,† an admirable observer, contributed to the histology of the Actinozoa in 1851. He corrected Erdl's mistake concerning the supposed striation of the muscular fibrillæ of the tentacles, and also Quatrefages and Leuckart's notion concerning the rupture of the tentacular ends previously to the passage of water from them. Giving very good illustrations, he proved himself to be a very reliable investigator.

Amongst other parts of the Actinozoa, he paid especial attention to the minute anatomy of the "bourses calicinales." These bead-like appendages, situated just without the tentacles in some genera, but

---

FIG. 12.—Three portions of intermediate tissue ending in the layer of granular cells which underlies the Rötteken bodies.
" 13.—Haimean and Rötteken bodies and the intermediate tissue in position.
" 15.—A diagram, but very close to nature, of the relative position of the histological elements of the chromatophores.
" 16.—Haimean and Rötteken bodies intermingled.
" 17.—Haimean bodies surrounded by pigment cells, and with bacilli flat upon them, owing to pressure.
FIGS. 18 and 19.—Fusiform nerve cells.
FIG. 20.—A nerve cell.
" 21.—Nerve cells connected and with fibres.
" 22.—A spherical nerve cell with processes joining the plexus.
" 23.—Ramifications of the plexiform cord.
" 24.—Nerve cell and fibres.

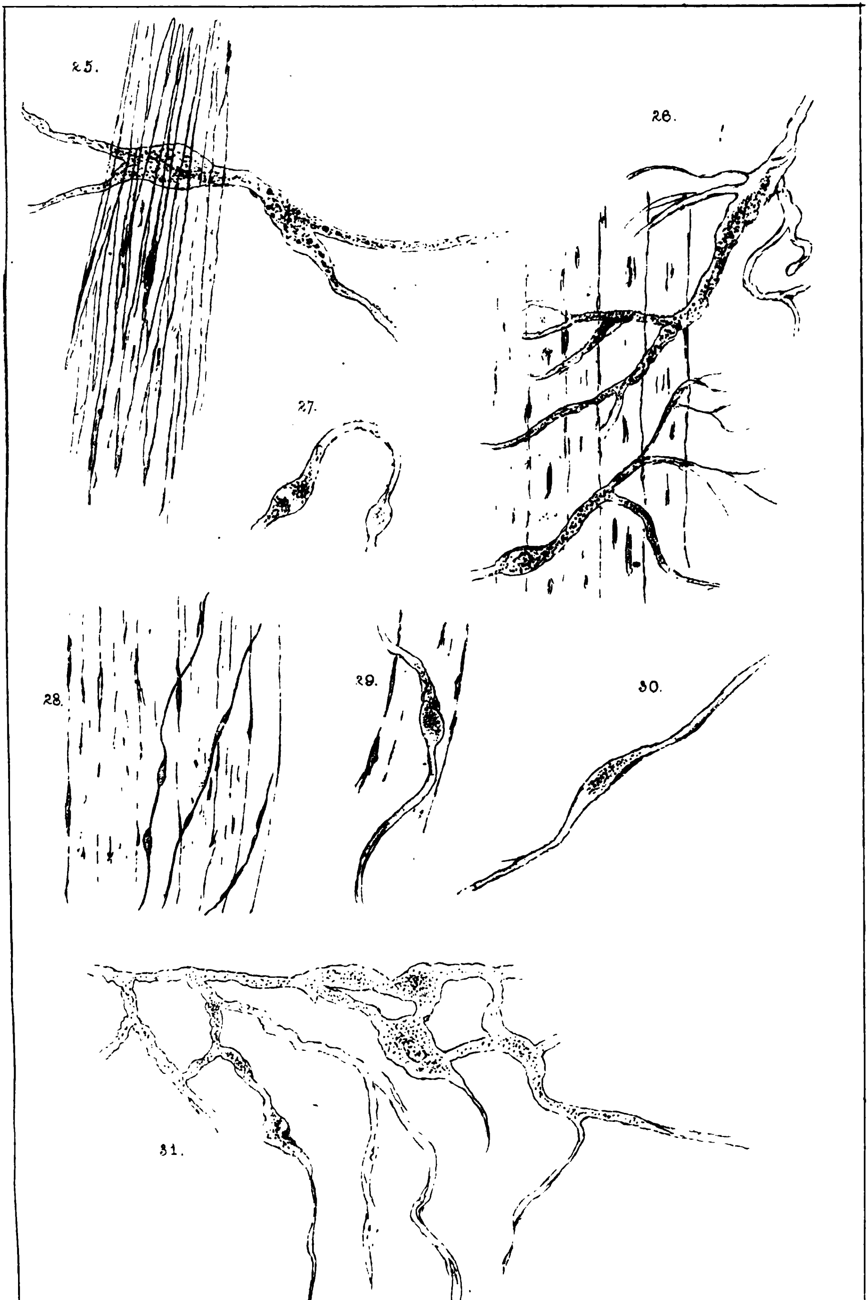
PLATE LXX.

" 25.—Nerve in relation to the small muscular fibrils of the base.
" 26.—Nerve ramifying and supplying wide muscular fibre.
" 27.—A loop of nervous fibre.
" 28.—Terminal ends of the plexus passing over muscular fibre.
FIGS. 29 and 30.—The same, more highly magnified.
FIG. 31.—The plexus under the endothelium.

---

* Huxley, 'Elements of Comparative Anatomy,' p. 82. See Dr. Grant, F.R.S., &c., on *Beroë pileus*, 'Zool. Trans.,' vol. i., p. 10. See also 'A Manual of the Subkingdom Cœlenterata,' by J. R. Greene, B.A., 1861, p. 165.

† "Sur les Actiniæ," 'Ann. des Sciences Nat.,' 1851.



P. M. D. ad nat. del.
W. H. Wesley lith.

W. West & Co. imp.

Nervous system in Actinia.

not in all, are also called chromatophores and "bourses marginales"; and their beautiful turquoise colour had rendered them attractive to previous anatomists, who had, as has already been noticed, guessed concerning their function.

Homard determined that they were folded elements of the skin in which the capsules (nematocysts) were enormously developed. He stated that the thread of these gigantic nematocysts was seen with difficulty. He noticed the transparency of some large cells in the bourses, and stated that, in his opinion, there was "some physiological relation between these little organs and the light."

Jules Haime (probably in 1855) examined the minute anatomy of *Actinia mesembryanthemum*, and his colleague, Milne-Edwards, quotes him in the 'Hist. Nat. des Coralliaires,' vol. i., p. 240. The lamented young naturalist found out that the chromatophores bore, so far as their number is concerned, a decided numerical relation with the number of the tentacles. He decided that they contained but few muscular fibres, and had navicular-shaped nematocysts, "diversement contournés," with indistinct threads within them. However, he recognized large transparent cells and pigmentary granules in them. The nematocysts of the chromatophores are larger than those of the tentacles. He was evidently not satisfied with the data upon which these coloured masses were decided to be of importance as organs of special sense. In all probability Haime was aware of Homard's work.

Kölliker and the German histologists added about this time, and later, to the exact knowledge respecting the histology of the muscles, skin, endothelium, and tentacular apparatus, but no advance was made towards the discovery of a nervous system in the *Actinia* for many years.

In 1871 the popular idea of the extent of the nervous system in *Actinia* was expressed by Alex. Agassiz,* who wrote:—"Notwithstanding its extraordinary sensitiveness, the organs of the senses in the *Actinia* are very inferior, consisting only of a few pigment cells accumulated at the base of the tentacles."

But in this year a great advance was made towards discovery by Profs. A. Schneider and Rötteken.† The first-named naturalist paid especial attention to the development of the lamellæ and septa in Corals and Actiniæ, and his colleague laboured in the histology of *Actinia* especially.

Working at a very great disadvantage, with specimens which had been preserved in alcohol, Rötteken produced a series of researches which added greatly to the knowledge already granted to

* 'Sea-side Studies,' Eliz. and A. Agassiz, 1871, p. 12.

† Sitzungsbericht der Oberhessischen Gesellschaft für Natur- und Heilkunde, March, 1871 (On the Structure of Actiniæ and Corals). Translated for the 'Ann. and Mag. of Nat. Hist.,' 1871, vii., p. 437, by W. S. Dallas, F.L.S., &c.

science by Homard and Haime. So far as they bear on the nervous system, the result of his researches may be stated as follows:—"The bourses marginales" (chromatophores) are undoubtedly organs of sense, and, indeed, compound eyes. They are pyriform diverticula of the body-wall, standing between the tentacles and the outer margin of the peristome; they are constructed after the fashion of a retina, and the following layers of structure may be distinguished in them:—1, externally a cuticular layer broken up into "bacilli" by numerous pore-canals; 2, a layer of strongly refractile spherules, which may be regarded as lenses; 3, cones—hollow, strongly refractile, transversely striated cylinders or prisms rounded at the ends; these have hitherto been confounded with urticating capsules (nematocysts): at the exterior end of each cone there is generally one lens, and sometimes two or three may stand in the interspaces; 4, a granular fibrous layer occupying the interspaces between the cones; 5, a layer which is deeply stained by carmine, and contains numerous extremely fine fibres and spindle-shaped cells, probably nerve fibres and cells; 6, the muscular layer; 7, the endothelium, which bounds the perigastric cavity.

Actinia mesembryanthemum was the species examined, and the diagram (Pl. LXIX., Fig. 15) will explain the relative position of the layers.

Rötteken could not determine the position of the pigment of the chromatophores from the alcoholized specimens. An examination of the minute anatomy of the tentacles of *Actinia cereus*, Ellis and Solander, determined that the refractile spherules and large cones were to be found on the tips of these organs.

Dana,* in his popular work on Corals and Coral Islands, appears to accept the statements quoted above. He states that "they sometimes possess rudimentary eyes"; and elsewhere, "they have crystalline lenses and a short optic nerve." He then observes:—"Yet Actiniæ are not known to have a proper nervous system; their optic nerves, where they exist, are apparently isolated, and not connected with a nervous ring such as exists in the higher Radiate animals."

II. *A Description of the Morphology of the Chromatophores.*

During the summer of 1871 the author of this communication was examining into the minute anatomy of *Actinia mesembryanthemum*, and had the advantage of possessing living specimens. Having satisfied himself of the general correctness of Rötteken's admirable work, he relinquished the inquiry until 1873, when he resumed it.

Everyone who has endeavoured to anatomize one of the Actiniæ

* 'Corals and Coral Islands,' by James D. Dana, LL.D., 1872, pp. 39, 41.

must acknowledge the excessive difficulties which accompany the attempt. The irritability of the muscular tissues, their persistent contraction during manipulation, the confusion caused by the abundance of different cellular histological elements, and the general sliminess of the whole, render the minute examination very troublesome and usually very unsatisfactory. Reagents are useful for rough examinations; but when the most delicate of the tissues are to be examined they must be floated under sea-water, and this must be the medium in which they must be examined under the microscope. Carmine solution, osmic acid, and spirits of wine in weak solutions are useful after the natural appearances have been determined, but they exaggerate some histological elements and destroy others.

Great care must be taken in making the thin sections, and no tearing must be allowed; for it is of paramount importance, in endeavouring to trace the nervous system, that the relative position of parts should be retained.

It is useless to rely on any observation made with object-glasses lower than $\frac{1}{18}$-inch focus (immersive).

In examining the chromatophores, Actiniæ with very bright-coloured ones, and other specimens with these organs dull in tint, should be selected. Fresh subjects should be obtained, and it is not necessary to kill them first of all. The blades of very delicate scissors should be allowed to touch the desired chromatophore close to its base, and then as the *Actinia* commences to contract, they should be brought together gently and without wrenching the tissues. By this method the chromatophore will remain on the blades. Two or three chromatophores may be removed, with their intermediate tissues, without injury to the animal; but, of course, the excision must not be too deep, or the endothelium will be cut into.

A dropping tube should be used to wash the chromatophore off the blades on to a glass slide, where a drop of sea-water awaits it.

Sections are by no means easy to make, but they are best performed under a power of 10 diameters with fine scalpels. The forceps must not be employed, as it crushes the tissues. If possible, very slight pressure should be exercised on the thin glass, which is to be placed very carefully and wet over the object. After the examination, carmine should be added, or osmic-acid solution, 1 per cent. in strength; but no results can be relied on which are derived from the examination under the influence of reagents alone, as they modify the natural appearance greatly.

So far as the chromatophores are concerned, my investigations took the following course:—1. Rötteken's researches on the alcoholized *Actinia* were followed in recent specimens. 2. The tissues of the chromatophores, of their margins, and of the spaces between

them were examined in a large specimen of a living pale-green variety of *Actinia mesembryanthemum* from the Mediterranean. 3. The tissues of the chromatophores of the *Actinia mesembryanthemum* were again examined with a view to explain the differences between M. Rötteken's and my own results.

The rounded, free, coloured, external layer of a chromatophore was carefully disengaged from the granular tissue beneath it, so that the bacilli of Rötteken, the refractile corpuscles, and his so-called cones were separated from the rest. This turquoise-coloured film was floated and carefully placed on a glass slide, the bacillary layer being inferior and on the glass, whilst the proximal ends of the cones were free in the water. No thin glass was placed over the film, and an object-glass of $\frac{1}{2}$-inch focus was used. The appearance presented under this low power (by transmitted light) was very remarkable, for a great number of brilliant points of light were seen surrounded and separated by dark opaque tissue. When a $\frac{1}{4}$-inch object-glass was used, the appearance was less striking, for the points of light were more diffused. No trace of an object could be seen through the refractile tissues.

The transparent and refractile tissues were the so-called bacilli, the globular bodies and the "cones" already noticed; and the tissue, which was impermeable by light, consisted of the colouring matter in small dull granules, cells small and round in outline and granular, and also the cell-walls of the cones.

Sections through a chromatophore were made at right angles to the point of the greatest convexity of the surface, and thin slices were floated off carefully from the line of section on to glass slides. The slices included (*a*) the coloured outside of the chromatophore, (*b*) the tissue beneath it, and (*c*) some muscular fibres which limit the endothelium. Sea-water was used as the medium, and a thin glass cover was applied after the specimens had been examined with a low power.

Externally was the bacillary layer (Pl. LXIX., Fig. 15). Rötteken describes this as a cuticular layer broken up into bacilli by numerous pore-canals. Examined, however, in the fresh subject, this external layer consisted of a vast multitude of small rod-shaped bodies, sharply rounded but conical at both ends, very transparent, and resembling the smallest nematocysts of the tentacles without the internal thread (Pl. LXIX., Fig. 2). These are placed side by side, and the external rounded end of each is separated by a small space from the terminations of its neighbours. These ends are free and are in contact with the water in which the *Actinia* lives. The rods are cylinders, and are separated from each other by a very delicate film of protoplasm, in which are numerous dark opaque granules and a few flat simple colourless rounded cells (Pl. LXIX., Fig. 3). The inner ends are shaped like the external, and are imbedded in the

next layer of tissue. Each of these bodies is a simple cell filled with a transparent fluid. When a thin film of the surface of a chromatophore is removed and examined under a $\frac{1}{16}$-inch, the bacilli may be observed to crowd together over a layer of large refractile cells. The thin glass cover is generally sufficient to crush down the bacilli, so that their sides may be seen as they rest in all kinds of positions on the deeper cellular layer (Pl. LXIX., Fig. 17).

The bacilli are not found universally over the chromatophores, nor do they invariably cover the layer of large refractile globular cells.

It will be noticed, on examining excised portions which include two or three chromatophores and their intermediate tissue, that not only are they marked on their surface by foldings of their superficial tissue, but that between them there are others which are microscopic. These last rarely have bacilli. Moreover, in some parts of the margins of the chromatophores, other pigments are visible than the turquoise, and the red often predominates; the bacilli are not usually present there.

Beneath the superficial layer of bacilli and their separating protoplasm, which is faintly granular, there is some granular tissue with a few small spherical cells containing granules, and the inner ends of the bacilli are imbedded therein (Pl. LXIX., Fig. 3).

This granular tissue is very thin, but it covers and dips down between the large refractile cells, which form the next layer (Pl. LXIX., Figs. 4, 13, 15, 16, 17).

These cells are more or less spherical; the cell-wall is very thin, and the contents are transparent, colourless, and refractile. Some have a pale grey tint, and one or more extremely faint nuclei are attached to the inner surface of the cell-wall. The ovoid shape is occasionally seen.

These large cells, which transmit light so readily, are universally found on the chromatophores; and when there are bacilli upon them, the spherical shape is common.

At the margins of the chromatophores, and where the red pigment commences, these refractile cells assume much larger dimensions and more irregular shapes. These refractile cells are, as has already been noticed, imbedded in a tissue of granular and slightly cellular protoplasm, and this occasionally is differentiated into some peculiar structures.

Where there are no bacilli this granular tissue is increased in thickness and becomes superficial; moreover the granules then contribute to the colour of the chromatophore, and probably they always do so to a certain degree.

The refractile cells are not invariably confined to the layer above the so-called cones of Rötteken, although they are often thus limited in their position, especially if there are bacilli covering them. In

parts of the same chromatophore, where this apparently normal arrangement is seen, and especially on the microscopic chromatophores between the larger kinds, the large refractile spherules are found between and in the midst of groups of the cones (Pl. LXIX., Fig. 16).

In the chromatophores there is considerable variety in the size of the refractile cells; they appear to be developed from the small cells with a circular outline, which contain a few dark granules, and which are found in considerable abundance amidst the enveloping granular tissue (Pl. LXIX., Fig. 8).

The most striking of all the histological elements of the chromatophores are the cones of Rötteken, or the nematocysts with imperfectly visible threads of Homard. They are divisible into three series:—

a. Elongated simple cells, cylindrical in shape, with rounded and somewhat pointed extremities, consisting of a tough cell-wall which is capable of being bent without being broken or ruptured, and of colourless transparent contents which are rather viscid (Pl. LXIX., Fig. 5). They are four or five times the length of the bacilli, and three times their width. The cell-wall is faintly tinted with the peculiar colour of the chromatophore. These elongated cells are not conical, nor can they be really termed cones with any propriety; when observed through their greatest length, or when the light traverses their long axis, the cell-wall appears dark and the centre very refractile. They exist in vast multitudes over most parts of the chromatophore, and also in the intermediate tissue and its microscopic chromatophores.

β. Cells of the same shape as "*a*," but the cell-wall is faintly striated, the appearance being very distinct under a power of 2000 diameters (Pl. LXIX., Fig. 6). These cells are very numerous, and were noticed by Rötteken; they appear in the same position, and often amongst the cells with simple walls.

γ. Cells of the same shape and size as "*a* and *β*," with a well-developed thread within them, which usually has no barb (Pl. LXIX., Fig. 7).

These cells are common where there are no bacilli, but they occur here and there in all parts of the chromatophore circle.

In some rare instances the "Rötteken bodies" (for thus I would name these remarkable cells) are closely approximated, side by side, without the intervention of any structure; but, usually, there is a very thin layer of granular protoplasm, containing small cells, between them.

As the bodies are cylindrical and more or less closely applied by their sides, there is more space between them in some places than in others; and it is in these spots, where the bodies cannot come in direct contact, that their intermediate structures are elongated

and filiform (Pl. LXIX., Figs. 9–14). The filiform arrangement of the granulo-cellular protoplasm is often branched, and a set of elongated masses may unite above or below the bodies. The cells of this intermediate tissue are small and usually spherical; in one kind there is a large refractile nucleus, but in the commonest varieties the cells simply contain granules. It is necessary to study this tissue, because of its close agreement to what I presume to be the nerve structure, in some, but not in the essential, points. This tissue is clearly continuous with that which has already been noticed as separating and bounding the larger refractile cells outside the Rötteken bodies, and it is continued amongst the small closely-set granular cells which underlie these interesting histological elements (Pl. LXIX., Fig. 13).

The intermediate tissue binds together the bacilli, for it is continued upwards and between them, the large refractile cells (which I propose to term "Haimean bodies"), and the "Rötteken bodies," and it becomes lost in the cells upon which the proximal ends of these last rest.

It contains the granular structures which give, in the mass, the colour to the chromatophore, and it is evident that the Haimean bodies are developed from it.

The proximal ends of the Rötteken bodies retain their sharp and rounded contour amidst the dense layers of small granular cells which everywhere underlie them.

Those granular cells form a tissue through which light passes with difficulty under the microscope. They are regularly placed in series near the Rötteken bodies; but deeper they become less so, and then other anatomical elements may be observed between them and the muscular fibres upon which the whole chromatophore rests, and which in their turn limit externally the endothelium.

III. *A Notice of Rötteken's discovery of Fusiform Cells and of the different appearances of the Nervous Elements now first observed in the "Plexiform Tissue."*

Rötteken describes these nervous elements as extremely fine fibres and spindle-shaped cells, and asserts that they are probably nerve fibres and cells. But he has not traced them in conjunction, nor have the fibres been seen of sufficient length to anastomose.

I have found the fusiform bodies and their long ends—the fine fibres mentioned above. Moreover the connection of these irregular-shaped cells has been determined in these investigations, and the anastomosis of their processes and their connection with parts of a plexiform nervous tissue also.

These structures are in the midst of a mass of viscous proto-

plasm, granules, and granular cells, which merge gradually into the close layers of granular cells under the Rötteken bodies, and they transgress here and there on those layers.

The fusiform cells are numerous (Pl. LXIX., Figs. 18–24), and may be divided into two kinds:—(*a*) Those with irregular shapes and short terminal processes, which are prolongations of the cell-wall and are rounded off. These cells contain either highly refractile nuclei, or several nuclei with granular nucleoli. The fusiform shape is not invariable, and in Plate LXIX., Fig. 20, a large cell twice the diameter of a Rötteken body is seen amidst the granular plasm. It has a tail-shaped prolongation and some highly refractile nuclei.

β. Those which are rounded in outline, and whose projections are long and continuous with those of others. The outlines of these cells are soft, and without definite and sharp margins, and the colour is a very pale blue-grey. They contain one or more very distinct nuclei. Our type, illustrated in Plate LXIX., Fig. 21, has its cells rather wider than a Rötteken body, and they are connected by a process with sharply-defined wells—the cell, with many nuclei, having a long caudal fibril of a pale grey colour and rather sharp marginal lines which had suffered disruption.

A second type has large spherical or elliptical cells, which do not have processes passing out in opposite directions, but they are restricted to one part. Usually the cells have only one process, but sometimes two exist close together (Fig. 22).

These cells are granular within and have very indistinct nuclei; the cell-wall is extremely delicate, and the whole is of a pale grey colour. The fibrils of these cells are particularly connected with the plexiform tissue. In Plate LXIX., Fig. 22, there is a cell with two fibrils—one is short, for it dips down and is foreshortened, and the other is very long; it bifurcates, and one end joins a rounded mass of the plexus, and the other the rugged fibrillar part.

In Plate LXIX., Fig. 24, a cell with one fibril is shown. The fibril swells slightly, and then passes down to join a transverse fibre belonging to the plexus.

The plexiform tissue is probably continuous around the *Actinia* beneath the chromatophores, for it is found between the circular band of muscular fibres and every chromatophore. It consists of an irregular main structure and of lateral prolongations, which either anastomose with the fibrils from the fusiform and more spherical cells, or are directly continuous with the cells (Fig. 23).

The main structure resembles, in its indistinctness of outline and its pale grey colour and indefinite marginal arrangement, the fibre of the sympathetic of mammals, but it is less coherent and smaller. The usual appearance (Pl. LXIX., Fig. 23) is that of a

grey film with definite branches, and the whole has few granules here and there and a very few nuclei. It is intimately associated with the surrounding cell structures, but they may be separated by accident or compression. Here and there the structure enlarges and a ganglion-like cell is seen (Pl. LXIX., Fig. 22).

I have traced this structure almost across the whole field of the microscope in some sections.

It appears that this portion of the nervous system of *Actinia* (namely, the fusiform and spherical cells with fibrils and the plexiform structure) is distinct histologically from the fibrillar and cellular structures amidst the Haimean and Rötteken bodies. These structures are connective and developing; but it must be remembered that it is possible for both series to come in contact in the midst of the layers of granular cells which underlie the Rötteken bodies.

IV. *Examination into the Physiological Relation between the Chromatophores, the Nerves, and Light.*

The question arises, Are these nerves of special sense? MM. Schneider and Rötteken answer that the small portion of the nervous arrangement they described, i. e. the fusiform bodies and their fibrils, are optic nerves. They are satisfied with the physical arrangement of the bacilli, Haimean and Rötteken bodies, and the nature of the colouring matter imitating that of an organ of vision.

The discovery of the anastomosing fibrils and the plexiform arrangement favour this theory; but there are reasons to be considered which throw much doubt on the views of the distinguished investigators. All *Actinia* have not chromatophores, and closely allied genera may or may not have them. Thus, amongst the *Actinia* with smooth tentacles, there is a group with non-retractile and another with retractile tentacles: amongst those with non-retractile arms are the genera *Anemonia* and *Eumenides* without chromatophores, and *Comactis* and *Ceratactis* with them; amongst the *Actinia* with retractile tentacles are *Actinia* with, and *Paractis* without, chromatophores.

Amongst the tubercular division, the genus *Phymactis* has chromatophores, but its close ally *Cereus* has them not.

Whatever may be the value of this classification of the *Actinia*, it is quite evident that to group together those with and without chromatophores in separate divisions would be the reverse of producing a natural arrangement. It is therefore difficult to believe that these ornaments, with something resembling an optical arrangement, can be the seat of special sensation.

MM. Rötteken and Schneider have observed the large refractile

Haimean bodies in the tentacles, and, as will be noticed farther on, I have found them of enormous size in the peristome.

They are surrounded in those places, but not covered, with pigment cells and granules, and are situated just beneath the nematocyst layer in the tentacle, and beneath a corresponding layer, or one of bacilli, in the peristome. I have failed to recognize any nervous elements in the tentacles save the fusiform bodies, and there are none in the peristome except these irregular cells.

Again, the Haimean bodies are found in the chromatophores, in some places, amidst the Rötteken bodies, separating them.

Nevertheless it is true that light falling on the surface of an *Actinia* will reach farther into its structures where there are Haimean bodies, and farther still if the Rötteken cells underlie them. Where there is no pigment intervening between the bodies when placed side by side, or between the Rötteken cells, a diffused glare of light would impinge on the granulo-cellular layer below them, in which the nerves ramify and the nerve cells exist. But when the pigment granules and cells exist, they break up the general illumination and confine it to a series of separate bright rays. Each of them is brighter than the corresponding space of diffused light; and it would appear that the bacilli, the Haimean bodies, and the Rötteken cells in combination, concentrate light.

Two or three bacilli are placed side by side and behind each other over a small Haimean refractile spherical cell, and perhaps twenty or more cover a large cell (Pl. LXIX., Fig. 15). Usually a Haimean body is placed immediately over a Rötteken body; but, as Rötteken has pointed out, this is not an invariable arrangement, for some cover the spaces between and over them. The refractibility of the fluid contents of the Haimean bodies and Rötteken cells appears to be the same; but the elongated form of the last-mentioned structures may act upon light as if their internal fluid were more viscid.

In every instance there is a more or less opaque tissue between the proximal end of the Rötteken body and the nerve cells; and, moreover, the delicate protoplasmic layer, which is slightly impervious to light, surrounds the Haimean bodies.

In my opinion the Haimean bodies, wherever they exist, carry light more deeply into the tissues than the ordinary epithelial structures. This is also the case with the bacilli and Rötteken bodies, even when they exist separately and with or without the Haimean bodies. There are three ordinary constituents of the skin, and through their individual gifts and structural peculiarity they place the *Actinia* in relation with light. When they are brought together in this primitive form of eye, they concentrate and convey light with greater power, so as to enable it to act more generally on the nervous system—probably not to enable the distinction of objects,

but to cause the light to stimulate a rudimentary nervous system to act in a reflex manner on the muscular system, which is highly developed. The *Actinia*, therefore, may feel the light by means of the transparent histological elements when they are separate and constitute integral portions of the ectoderm; but this sensation will be intensified when the three kinds of cells are placed in such order as has been observed in the chromatophores.

The evolution of an eye, which can distinguish outlines, shadows, and colours, probably took the path which is thus faintly indicated in the *Actinia*, which doubtless has an appreciation of the difference between light and darkness.

V. *On the Nerves of the base of* Actinia mesembryanthemum.

A large specimen of a pale green variety from the Mediterranean was examined.

The base being free and expanded, a rapid incision cut out a triangular piece comprehending the ectothelium, the muscular layers, and the mucous endothelium. The apex of the triangle reached the centre of the base of the *Actinia*, and the base of the triangle, which was covered, corresponded with the basal margin of the animal.

Sections were made parallel with the original aspect of the base of the *Actinia*, and then some others at right angles.

The histological elements were studied separately and compared, so that the following tissues could be distinguished readily:—

1. A fibrous-looking tissue like ordinary white fibrous tissue with dark nuclei, to which the muscular fibres are attached and from which they originate.

2. A dense layer of muscular fibres, or rather fibrils, which originates at right angles to the fibre of the fibrous tissue. Each fibril is refractile and nucleated. Each is separate from its neighbours, and lies in the midst of granules and small cells which contain granules, all being highly refractile. In some places the fibrils are gathered together in masses, so as to leave areolæ between them.

3. Large muscular fibres in contact laterally, so as to form a thin layer. Each fibre is long, broad, has several pale elongate nuclei and a distinct lateral dark line. There are no striæ.

4. The elements of the endothelium and ectothelium, which, as they do not bear on the immediate subject, will be described in a future memoir.

The object of the investigation being to discover some trace of a nervous system, which was presupposed to resemble somewhat the traces observed below the chromatophores, the necessity of becoming familiar with the fibrous and muscular tissues, so as to decide what was not muscle and fibre, is apparent.

I have not found any isolated fusiform cells amongst the tissues of the base; but under the endothelium, and also between the layers of muscular fibres, there are structures which I feel disposed to believe must belong to the nervous system. 1. They are in the position of nerves. 2. Their structure is not that of muscle or fibre. 3. Their structure resembles, in some instances, the plexiform tissues beneath the chromatophores.

The nervous structures are found to present three characteristic shapes:—

1. A thin layer of muscular fibrils of the small and separate (see 2 above) kind, with well-defined dark nuclei in them, was examined. The whole was very transparent and well defined under the $\frac{1}{18}$-inch objective.

Underlying this layer, and extending on either side beyond it, so as to appear in one of the meshes between groups of these fibrils, was a ramified pale grey tissue, which was less pervious to light than the muscular fibrils (Pl. LXX., Fig. 25). Swollen in one part and faintly granular throughout, it had its margins very faintly visible. It was flat, and had a definite resemblance to the widest portion of the plexus already mentioned.

2. A large section of muscular tissue was examined. It consisted of one layer of large muscular fibres (see 3 above) in close lateral contact. Running obliquely over the layer was an irregular but continuous cord ramifying here and there, the branches breaking up into fibrils. In one part the cord was swollen (Pl. LXX., Figs. 26 and 27). A second ramification passed from the opposite end of the field of the microscope and broke up into ultimate fibrils, and in this structure there was a fusiform cell.

Careful manipulation separated a portion of the upper cord from the muscular fibres, but a part of it evidently dropped down amongst them.

3. A layer of muscular fibres of the same kind as those just mentioned was examined. It was marked, as usual, with the lateral dark lines and pale elongated nuclei.

Three long and irregular fibres passed more or less obliquely over the muscular tissue (Pl. LXX., Figs. 28–30). They had distinct lateral or marginal lines, were swollen out in several places, and their texture was faintly granular.

I believe that these fibres were continuous with the fine ramifications of the plexiform arrangement just described.

4. Above the muscular layers, and under the folds of the endothelium, I found an inosculating series of ramifications arising from a common cord. It was situated upon the layer of muscular tissue, with small and separate long fibrils.

The structure was faintly granular, pale grey in colour, with faint outlines, and was swollen in some places: it covered a con-

siderable portion of the field of the microscope; and portions of it had a close resemblance to the ramifying structure mentioned as having been observed below the muscular layer (Pl. LXX., Fig. 31).

The multiplication, if it be justifiable, of these structural elements in the other segments of the base which were not examined would give a fair notion of the plexiform arrangement of the basal nervous tissue. I presume that it consists of a reticulate structure beneath the endothelium, which sends large branches between the vacuities of the most delicate muscular layer, and which communicates with a ramifying tissue in contact with the other muscular layers, and that this ends in long fibres which supply the wide fibres of this last-mentioned layer.

The diffused nature of this nervous tissue is what might be anticipated would be found in animals possessing such general irritability of tissue, and probably its function is to assist in the reflex movements of the animal, and to produce expansion of the disk on the stimulus of light.—*Proceedings of the Royal Society*, vol. xxii., No. 151.

IV.—*On Diapedesis: or the Passage of Blood-corpuscles through the Walls of the Blood-vessels, and how to observe it.*

By Joseph Needham, F.R.M.S.

The importance of the subject of diapedesis cannot be overrated, for one can scarcely attend a course of lectures on physiology, pathology, surgery, or medicine, without hearing, over and over again, a description of, or a reference to, this process; surely, then, you will not regard the time spent in verifying for yourselves so important a fact as wasted. Before proceeding to the substance proper of my discourse, I wish to observe that, although I may draw now and again on physical facts, or encroach too much on the domain of pathology, yet those digressions will only be made to render the explanation of the subject more lucid, or to testify to the importance of a right conception of it.

We will, for convenience, consider our matter under four headings.

1. Mode of demonstration.
2. Description of process.
3. Explanation of phenomena.
4. Concluding remarks.

I. As to the *modes of demonstration*, various observers have made use of different animals. The material has been drawn chiefly from the Batrachians and their tadpoles. Fish have occasionally been employed; recently warm-blooded animals have been

used; but the study of the capillary circulation of mammalia is attended with such great difficulty that they are seldom employed.

We will dismiss the fish and mammalia in a few words. An oblong box of gutta-percha, with glass top and bottom, is generally employed for studying the circulation in fish. A constant supply of fresh water is conveyed to, and the deoxygenated water from, the box, by means of two pipes. As regards mammalia, there is but one external part—viz. the wing of the bat (used by Paget)—sufficiently transparent for observation; hence it is necessary to employ some internal structure. The mesenteries of small rodents have been used by Stricker; but these are not to be compared with the omentum, and particularly with that of the guinea-pig. It differs from that of man in consisting of only two layers of peritoneum, in being much more delicate in structure, and containing very little fat. The observations on these serous membranes have been productive of no good results, for the injurious effects of exposure are much greater than those which occur in Batrachians.

Therefore, to overcome these difficulties, it is necessary to have recourse to complicated appliances and expensive apparatus, and even then so vulnerable a tissue as that of the peritoneum cannot be exposed even for a few minutes without injury, so that, although the greatest care is taken in demonstration, only a momentary glimpse can be obtained.

Batrachians and their larvæ. Four parts of these animals may be used, as follows:—(1) The web of the frog's foot. (2) The mesentery of the frog or toad—preferably of the toad, whose mesentery is longer. (3) The tongue of the toad or frog. (4) Tadpole's tail. These animals may or may not be curarized. If curara be employed, one drop of one-sixth per cent. solution is injected under the skin of the back with an ordinary hypodermic syringe. If not curarized, the brass frog-plate sold by opticians, or a large flat piece of cork with a hole at one end, must be employed; but these arrangements are only fitted for examining the web.

1. The web of the foot must be extended over the glass in the brass plate, or the hole in the cork, by means of thread tied to the toes and fastened to the plate, or by means of pins to the cork; but the web is not well fitted for observing diapedesis. The epithelium soon becomes dulled by the action of reagents, and often the extension of the web impedes or entirely prevents the circulation.

2. The mesentery. Although the preparation of the mesentery is not so simple as one might anticipate, yet it is well suited for our purpose. The animal must be under the influence of curara, and the use of a stage frog-plate is necessitated, the construction of which is as follows:—A piece of cork, having a circular hole in the middle, is fastened to one end of a plate of glass or cork with a corresponding hole; a circular piece of thin glass is fixed to the

projecting cork, above the hole, partly covering its surface. An incision is made parallel to the median line on the right side of the belly. Care must be taken not to sever any large vessels. A similar incision is then made in the exposed muscles, still avoiding the blood-vessels. Should there be any bleeding from a large vessel it must be restrained by torsion or ligature. All traces of blood having been removed with bibulous paper, the intestines and mesentery are drawn out carefully and placed on the projecting cork. The animal must lie supine. The intestine is covered with bibulous paper, and a thin "cover" glass placed on the mesentery. The whole is kept moist by the frequent addition of a few drops of a ½ per cent. solution of chloride of sodium. The mesentery should be exposed for two or three hours before required for observation.

3. The tongue was first used by Cohnheim. The animal in this experiment must also be curarized. A plate, similar to that last described, is used. The tongue is drawn out and ligatured near its root. Forty-eight hours after, the ligature is removed, and the circulation generally recovers in a short time. Dr. Mitchell Bruce suggests interposing a piece of leather between the ligature and the tongué, to prevent injury to the organ. The animal is placed on its back, the tongue drawn forwards, spread over the thin glass, and secured in position by means of small pins. The dorsum of the organ will be directed upwards.

4. The tail of the tadpole can be arranged with great facility, and affords a most interesting object. Dr. Klein recommends curarizing the animal by placing it in a moderately strong solution of curara until motionless. It is then placed on an ordinary glass slip, the tail covered with a thin glass, and kept moist by the addition of salt solution when necessary.

II. The examination should be commenced with a low power, so that a moderate-sized vein can be easily selected. The low power is changed for a No. 7 Hartnack, corresponding to an English quarter inch. The chosen vessel must now be closely watched.

In every vessel, so long as the parts are natural, the central part of the current is occupied by coloured, and the periphery by colourless, corpuscles. The coloured pass along with their long axes parallel to the long axis of the vessel, whilst the colourless assume a spheroidal form, and move more slowly, or, as Dr. Burdon Sanderson has aptly likened them, to "round pebbles in a shallow but rapid stream."

A column of fluid may be supposed to consist of several strata, and the friction of the fluid against the wall of the vessel causes the external layer to move more slowly; hence the slow onward progress of the colourless, and the quicker transit of the coloured. After a while the relation will be seen to be altered; the vessel

becomes filled with coloured corpuscles, which have a tendency to reach the external layers of the fluid, whilst the colourless are adhering to the walls of the vessel. Frequently they are loosened and swept onwards by the current, but many remain stationary, and in a short time elevations will be observed on the outer side of the wall of the vessel. These gradually increase in size at the expense of the corpuscles on the interior, until at last the perfect corpuscles are seen wandering away from the vessel into the tissues by means of their delicate hyaline processes or pseudopodia.

III. We will now consider the fallacies, and the various explanations, of these phenomena.

Fallacies.—One great source of error is the rupture of some vessel accidentally or from operation. The blood-corpuscles spread over the field of the microscope, and a false impression is conveyed to the mind of a superficial observer, viz. that they have escaped from the vessel under observation; but careful focussing will soon reveal the mistake. Very often a small vessel may partly cover, or be concealed by, a corpuscle. Again, the observer imagines he sees a corpuscle emerging from the vessel; but no errors of this description can ever be committed if the following simple rule be rigidly observed, viz. that all corpuscles on their passage through the capillary walls consist of three distinct portions—one on the exterior, another on the interior, and a third or neck uniting them; in fact, a distinct constriction should be seen.

Explanations.—Observers do not agree on the manner in which the corpuscles escape from the unruptured vessels.

Cohnheim considers that by virtue of the amœboid motion they possess, their exit is readily effected through the false stomata between the endothelial elements of the vessel. He also states that the molecular arrangement of the constituents of the endothelium is altered; hence it becomes sticky or viscid, and the onward course of corpuscles is retarded. Billroth believes that it is due to some chemical or some molecular changes producing softening of the vascular walls.

Cohnheim stated that pressure assisted materially in the process; but if the arteria-media of a rabbit's ear be exposed, the corresponding vein opened, and distilled water injected through the artery at a lower pressure than that of the blood, it will produce inflammation, although no fluid has been expressed from the vessels.

The introduction into the circulation of a small quantity of 2 per cent. solution of common salt will also produce an abundant migration.

Stricker and Prussak consider the process due to an "active state" of the walls of the vessels, which consist of a homogeneous extensile protoplasm, and adduce as proofs that all colloid substances allow other colloid substances to pass through without their

integrity being broken, as proved by Graham, and have likened the process to the passage of frog's blood-corpuscles through the much smaller interstices of a fine filter. Erichsen thinks that the nerves and other tissues have an important influence.

In silver stained preparations the corpuscles always lie in the inter-endothelial lines.

The coloured corpuscles, which can also be observed to migrate, are either taken up by the colourless corpuscles, or are disintegrated, producing pigmentation.

IV. Intimately connected with this subject is the origin of pus. We now know that migrated blood-corpuscles form the chief source; but, if we carefully review the subject, we shall agree with Dr. Payne's remarks on the subject before the Medical Microscopical Society, viz. that "Virchow's idea of the origin of pus, though now old-fashioned, is far from being overturned by Cohnheim," for the observations of Stricker and Recklinghausen go directly to prove origin, in part at least, by proliferation of connective-tissue corpuscles.

NEW BOOKS, WITH SHORT NOTICES.

On Spectrum Analysis as applied to Microscopical Observations: the subject of a Lecture delivered at the South London Microscopical Club. By W. T. Suffolk, F.R.M.S. London: John Browning, Strand, 1873.—It will strike the reader with some astonishment that this book has not been noticed before, but the fact is that it was sent to the Editor's former address, and was there detained. Hence the delay. There is not of course very much to be said in critique, for the work is extremely elementary in character; still, in the absence of any cheap essay on spectroscopic operations, the microscopist will find in it all that he requires to make him understand the construction and principles of action of the spectroscope, and to enable him to perform a number of experiments with accuracy. Besides this, it really contains in the series of plates that accompany it, the spectra of most of the substances that have been examined up to the date of its publication. It is of course to be regretted that the colours of the spectrum were not in all cases given, but this is a trifling fault to find, the more so when the cheapness of the little volume is concerned. Besides, the frontispiece consists of well-coloured spectra of the sun, of ten of the elements, of one of coal-gas, of one of Sirius, and of one of the nebula. The author first gives a popular account of the peculiarities of spectroscopy, and explains, we think very satisfactorily, what is a puzzle to some people, viz. the peculiarity of the bright lines becoming black ones under certain conditions. His explanation of the apparatus is clear also, and is of course confined to micro-spectroscopy; and his account of the different spectra is lucid and to the point. Lastly, he gives a useful list of the several papers which have been contributed on the subject of micro-spectral work to English journals. We have no fault to find with, but a good deal of praise to award to, Mr. Suffolk for his useful little volume.

On the Origin and Metamorphosis of Insects. By Sir John Lubbock, Bart., M.P., F.R.S., with numerous illustrations. London: Macmillan and Co. 1874.—Of the many workers on the subject of entomology which we possess in this country, there is none who has a greater right to come forward as one thoroughly qualified to speak on the complex questions relating to the development of insects than Sir John Lubbock. We fancy that many people, even among those with zoological tastes, are not aware how thoroughly qualified is the author of the present treatise for the work he has taken in hand. We think, therefore, that he was quite wise in publishing a list of his papers on this subject. And from this list we see that he has written no less than thirty-five valuable memoirs on this subject, many of them being contributions to the Royal Society of London, and all extending over a period of twenty years. It may be said, then, that whatever the views he expresses, they have not been hastily formed; and from the nature of his writings, having to do chiefly with the structure and development of insects, they are entitled to every confidence. For ourselves, we must say that we have read the work with a great deal of pleasure,

both from the marked clearness of the writer as an instructor, and from the excellent series of illustrations which it presents. But it would indeed be very poor criticism which confined its observations by such limits as these. There is a philosophical tone about the volume which is its highest quality, and it is this, we think, which will be most highly valued by the thinking reader, more especially if his tendencies be Darwinian. In fact, the author has tried the very difficult task of attempting a system of classification which will show the force of the theory of evolution as it applies to the class Insecta. He has endeavoured to show how the class was originally developed, and then to trace out its several modifications, and its relation to the neighbouring classes. And this he does, it appears to the writer, in a very successful fashion. Indeed, Sir J. Lubbock's appears to be unquestionably the most successful attempt that has been made in the application of Darwinism to the group he has taken in hand. He attempts to do for this group what Fritz Müller has so splendidly performed for the class Crustacea. But besides this general feature of the work, it is interesting from the number of remarkable passages it contains referring to the more peculiar habits of certain of the group. We shall quote one or two remarkable cases, and not the least important is that relating to the solitary hymenoptera. "The solitary bee or wasp," he says, "forms a cell generally in the ground, places in it a sufficient amount of food, lays an egg, and closes the cell. In the case of bees the food consists of honey; in that of wasps, the larva requires animal food, and the mother therefore places a certain number of insects in the cell, each species having its own especial prey, some selecting small caterpillars, some beetles, some spiders. *Cerceris Cupresticida*, as its name denotes, attacks beetles belonging to the genus Buprestis. Now, if the *Cerceris* were to kill the beetle before placing it in the cell, it would decay, and the young larva when hatched would find only a mass of corruption. On the other hand, if the beetle were buried uninjured, in its struggle to escape it would be almost certain to destroy the egg. The wasp has, however, *the instinct of stinging its prey in the centre of the nervous system*, thus depriving it of motion, and let us hope of suffering, but not of life; consequently when the young larva leaves the egg it finds a sufficient store of wholesome food." A not less remarkable, though more questionable fact, is that relating to the habits of Clavigers and Ants; but this we may pass over. And indeed, many other equally remarkable instances might be quoted if our space was illimitable. We shall therefore pass on to what we consider the important part of the little book before us. It is that relating to the origin of the Insecta; and this is of course a most difficult problem for the naturalist. As the author shows, Palæontology supplies very little evidence indeed. As far as it goes, however, it supports the idea that "the Orthoptera and Neuroptera are the most ancient orders," though it affords small testimony as to which is the elder of these two groups; and beyond this it is valueless as a means of research. It is, then, upon embryology and development that the author rests his several conclusions; for he points out that very many cases occur where insects are related in early life which have no connection whatever in

the mature condition. Sir J. Lubbock gives a plate which illustrates this in the same manner as Haeckel does with regard to Crustacea; and this very circumstance will show how difficult it is to place before our readers any fair explanation of the views of the author; for in many cases he simply points to the woodcut as bearing out his view of the close relationship, and in these pages of course we cannot follow him. He says that the stag-beetle, the dragon-fly, the moth, the bee, the ant, the gnat, and the grasshopper, although they differ much from each other, being dissimilar in size, form, colour, and in habits of life, have been proved by those naturalists who have followed Savigny's method to be "constructed on one common plan." And further, our author shows that other groups, as, for instance, Crustacea and Arachnida, are "fundamentally similar." In the author's words, "we find in many of the principal groups of insects, that greatly as they differ from one another in their mature condition, when they leave the egg they more nearly resemble the typical insect type, consisting of a head, a three-segmented thorax, with three pairs of legs, and a many-jointed abdomen, often with anal appendages. Now, is there any mature animal which answers to this description? We need not have been surprised if this type, through which it would appear that insects must have passed so many ages since (for winged Neuroptera had been found in the carboniferous strata), had long ago become extinct. Yet it is not so. The interesting genus Campodea still lives; it inhabits damp earth, and closely resembles the larva of Chloëon, constituting, indeed, a type which occurs in many orders of insects. It is true that the mouth-parts of Campodea do not resemble either the strongly mandibulate form which prevails among the larvæ of Coleoptera, Orthoptera, Neuroptera, Hymenoptera, Lepidoptera, or the suctorial type of the Homoptera and Heteroptera. It is, however, not the less interesting or significant on that account, since, as I have elsewhere* pointed out, its mouth-parts are intermediate between the mandibulate and haustellate types, a fact which seems to me most suggestive." From these observations we gather that the author supposes the group of insects, both mandibulate and haustellate, to have arisen from ancestors somewhat resembling the Campodea type; and hence this form is, as he states, of "remarkable interest, since it is the living representative of a primeval type, from which not only the Collembola and Thysanura, but the other great orders of insects have derived their origin." We think that in regard to the minor question of which particular form the class sprang from—always admitting that they did spring from some one form—the author's ideas have more in them than those of Professor Haeckel, though of course every weight must be given to the distinguished German's opinions. We may sum up the author's opinions expressed fully and clearly in this interesting and well-illustrated little volume, by stating that he believes the insects generally are descended from forms like the present existing genus Campodea; and finally, that these in their turn have been derived from a type resembling the living genus Lindia.

* 'Linnæan Journal,' vol. xi.

PROGRESS OF MICROSCOPICAL SCIENCE.

The American Hydræ.—A note has been read before the Society of Natural Sciences of Philadelphia by Professor Leidy on the two species of *Hydra* common in the neighbourhood of Philadelphia. One is of a light brownish hue and is found on the under side of stones and on aquatic plants in the Delaware and Schuylkill rivers, and in ditches communicating with the same. Preserved in an aquarium, after some days the animals will often elongate the tentacula for several inches in length. The green *Hydra* is found in ponds and springs, attached to aquatic plants. It has from six to eight tentacles, which never elongate to the extent they do in the brown *Hydra.* In winter this animal is frequently observed with the male organs developed just below the head as a mammal-like process on each side of the body. He had not been able to satisfy himself that these *Hydræ* were different from *H. fusca* and *H. viridis* of Europe. Professor Agassiz had indicated similar coloured forms in Massachusetts and Connecticut, under the names of *H. carnea* and *H. gracilis.* Of the former he remarks that it has very short tentacles, and if this is correct under all circumstances, it must be different from our brown *Hydra,* which can elongate its arms for three inches or more.

Remarks on Actinophrys Sol.—Some observations made by Professor Leidy at one of the recent meetings of the Society of Natural Sciences of Philadelphia are not without interest.

Professor Leidy, after describing the structure and habits of this curious rhizopod, said that he had recently observed it in a condition which he had not seen described. He had accidentally found two individuals including between them a finely-granular rayless sphere nearly as large as the animals themselves. These measured, independently of the rays, 0·064 mm. in diameter; the included sphere 0·06 mm. He supposed that he had been so fortunate as to find two individuals of Actinophrys in conjunction with the production of an ovum.

Preserving the animals for observation, on returning after an absence of three hours, the animals were observed connected by a broad isthmus including the granular sphere reduced to half its original diameter. Two hours later the granular sphere had melted in the isthmus, leaving behind what appeared to be a large oil globule and half-a-dozen smaller ones. The isthmus in the former time measured $\frac{1}{25}$ mm., at the later time $\frac{1}{28}$ mm. Shortly afterwards, the isthmus elongated and contracted to $\frac{1}{50}$ mm. on the left, while the right half, retaining the oil globules, remained as thick as before. At the same time the animals became flattened at the opposite poles. The latter subsequently became depressed, so that the animals assumed a reniform outline. The isthmus, now more rapidly narrowed and elongated, became a mere thread, and finally separated about one hour from the last two hours indicated. The oil globules were retained in

the right-hand individual, which, with the remaining projection of the isthmus, appeared broadly coniform in outline. In the left-hand individual all remains of the isthmus at once disappeared, and the animal appeared reniform in outline, but now contracting on the same side it assumed the biscuit form. The constriction rapidly increased, and in thirty minutes from the time of separation from the right-hand individual it divided into two separate animals presenting the ordinary appearance of *A. Sol.* Thus this second division took place in an opposite direction from the first. The right-hand individual, retaining the oil globules apparently unchanged, more slowly assumed the reniform outline, and then became constricted all around. The constriction elongated to an isthmus, in the centre of which were the oil globules. Three hours after the separation of the right-hand animal, the isthmus was narrowed to about half the diameter of the two new individuals which were about to be formed. At this moment other engagements obliged me to leave the examination of the animals. Six hours after, in the animalcule cage, I observed only half-a-dozen individuals of the *A. Sol.*

The Fresh-water Algæ of North America.—Students of our fresh-water algæ will find in the beautiful and interesting work of Dr. H. C. Wood, jun., says the 'American Naturalist,' 'A Contribution to the History of the Fresh-water Algæ of North America,' a ready means of identifying their specimens. It is a large quarto volume, with many coloured plates, and is taken from the Smithsonian Contributions to Knowledge.

Development of Ferns without Fertilization.—At a late meeting of the American Academy of Arts and Sciences, Prof. Gray communicated a paper by his former pupil, Dr. W. G. Farlow, now in Germany, on the development of ferns from the prothallium irrespective of fertilization, by a sort of parthenogenesis. The growth observed took place, not from an archegonium, but from some other part of the prothallium.

Migration of White Blood Corpuscles.—Dr. Thomas read, before the German Association of Naturalists at Wiesbaden, a paper on the migration of the white corpuscles into the lymphatics of the tongue of a frog, which is thus abstracted by the 'Lancet':—He injected the lymphatics of the living animal with an extremely dilute solution, not containing more than $\frac{1}{2000}$th to $\frac{1}{8000}$th part of nitrate of silver, and found that, with certain precautions, this did not lead to stasis of the blood in blood-vessels, but only to a lively exodus of the white corpuscles from their interior. After the lapse of some time, when the parts had begun to recover from the injurious effect of the injection, he was enabled to observe the re-entrance of the corpuscles into the lymphatic vessels, through certain stomata in their walls, now marked and rendered distinct by a precipitate of the silver salt. In a second series of researches the lymphatics were injected with a dilute emulsion of cinnabar, in a $\frac{3}{4}$ per cent. solution of common salt. The cinnabar was in part deposited in the stomata of the lymphatics, and partly passed through them, and was deposited in the tissues in the form of

small, round, cloudy patches. The evidence of the identity of the stomata, brought into view by means of the cinnabar, with those rendered evident by the nitrate of silver, is obtained by observing their peculiar grouping, and by the subsequent injection of nitrate of silver into the same vessels. The injection of the cinnabar causes very little disturbance of the circulation. If a lively exodus of the white corpuscles from the blood-vessels be produced by making an abrasion of the surface, the migrating cells quickly make their appearance in the stomata of the lymphatics marked out by the cinnabar. They then take up the particles of the cinnabar into their interior, which causes them to lose their activity and accumulate in the stomata. They then appear in the form of cauliflower excrescences, projecting into the interior of the lymphatics, which gradually break up into their constituent cinnabar-holding cells. These may be traced into the larger vessels, and from them into the blood. In these researches, a remarkable regularity, or uniformity, in the track pursued by the white corpuscles, was observed. They pass away from the blood-vessels nearly at right angles into the tissues, their course, however, being in a series of short zigzags. They all appear to travel about the same pace.

Persistence of Sensibility in the Peripheric Ends of Cut Nerves.—A paper on this by MM. Arloing and Tripier, is thus abstracted in the 'Medical Record,' June 17th, by Dr. B. MacDowal:—1. The facial and the spinal nerves of solipeds and rodents possess recurrent sensibility as well as those of carnivora.

2. To find recurrent sensibility most readily, one must go to the periphery.

3. The peripheric end of the branches of the trigeminus nerve is sensible. This sensibility is somewhat difficult to demonstrate; still it exists.

4. The peripheric end of the nerves of limbs is also sensible. The sensibility may, however, disappear towards the nerve trunks.

5. In any case, the sensibility of the peripheric end is due to the presence of nerve tubes, the relations of which with the trophic and perceptive centres have not been interrupted by the section.

6. The absence of these tubes implies sensibility of the peripheric end.

7. These tubes proceed from the fifth pair, for the facial; from neighbouring nerves, and occasionally from nerves of the opposite side, for sensitive nerves; from neighbouring and homologous nerves, for the mixed nerves.

8. These recurrent nerves rise more or less high in the trunk of the nerve to which they are connected; their number diminishes from the periphery to the centre.

9. The return of these fibres may take place before the termination of the nerves, but the termination is the part where it is made by preference.

10. For several reasons, MM. Arloing and Tripier think that the sensibility of the peripheric end belongs to all nerves; and that it probably exists in all animals of the class mammalia at least.

The Condition of Heart and Kidney in an obscure form of Disease, which lately occurred in America, is thus described by Dr. L. Curtis: —"The piece of heart presented on the outside simple atheromatous and calcareous degeneration. The muscular fibres appeared healthy. The kidney presented a mottled appearance, part being of a cream-colour, other portions being of a natural colour, except much paler. I took two small pieces of this kidney and placed them in a weak solution of chromic acid, to harden. After a day or two, I cut some thin sections, both in a longitudinal and a transverse direction, and stained them in an alkaline solution of carmine. On examining the sections with the microscope, the whole field appeared confused, and it was only after repeated and prolonged examination that I was enabled to make out anything at all satisfactory. This was particularly the case over the greyer portions. The cause of this indistinctness was the infiltration of the organ with a granular substance. In some places this granular substance was replaced by round bodies resembling, in size and appearance, pus corpuscles; in other places there were collections of round bodies from one-third to one-half the diameter of the former; neither of these collections had well-defined boundaries. The edges of some of the sections, which were extremely thin, showed, where the granular material had been washed out, that the connective tissue of the kidney was somewhat thickened, and contained many more muscular points than in health. The Malpighian tufts were, in many places, contracted down into little compact knots, of cicatricial-like tissue. The uriniferous tubules were filled with a granular material; the cells lining them had lost their distinctive characteristics, and were cloudy and opaque. Most of the straight tubules were wasted to mere irregular, nodulated cords. These appearances do not correspond altogether with any specimen that I have met before, or with any description that I have seen published. I should dislike, at present, to give a decided opinion as to their nature; they correspond, however, more closely with what Rindfleisch calls *cellular hypertrophy of the connective tissue*, than anything else with which I am acquainted."

On Tube-building Amphipoda.—In 'Silliman's American Journal' for June, 1874, Mr. S. I. Smith gives the following account. He says, "In examining recently an alcoholic specimen of a species of *Xenoclea*, I noticed a peculiar opaque glandular structure filling a large portion of the third and fourth pairs of thoracic legs, which in most, if not all, the non-tube-building Amphipoda are wholly occupied by muscles. A further examination shows that the terminal segment (dactylus) in these legs is not acute and claw-like, but truncated at the tip, and apparently tubular. In this species, a large cylindrical portion of the gland lies along each side of the long basal segment, and these two portions uniting at the distal end pass through the ischial and along the posterior side of the meral and carpal segments, and doubtless connect with the tubular dactylus. There can be no doubt that these are the glands which secrete the cement with which the tubes are built, and that these two pairs of legs are specialized for that purpose. A hasty examination revealed a similar structure of the corresponding legs in *Amphithoe maculata, Ptilocheirus pinguis, Cera-*

pus rubricornis, *Byblis Gaimardi*, and a species of *Ampelisca*. In all these except the last two a very large proportion of the gland is in the basal segment. In the *Amphithoe* this segment is thickened and the gland is in the middle. In the *Cerapus* it is very broad and almost entirely filled by the gland, with only very slender muscles through the middle, and the orifice in the dactylus is not at the very tip, but sub-terminal on the posterior side. In the *Ptilocheirus* the gland forms three longitudinal masses in the basal segment and is also largely developed in the meral and carpal segments. The dactylus is long and slender and the orifice sub-terminal. In *Ampelisca* and *Byblis* (which, like *Haploöps*, are tube-building genera) the meral segments of the specialized legs are nearly as large as the basal, and contain a proportionally large part of the gland. In these genera the remarkable elongation of the two distal segments in the third and fourth pairs of legs is perhaps a special adaptation to enable them to reach back over the deep epimera.

Retrogression of the Graafian Follicle.—M. Slavjansky has recently written a paper in the 'Archives de Physiologie,' which is thus abstracted in the 'Medical Record,' June 15th:—"1. The Graafian follicles are developed from the primordial follicles, and acquire a greater or less degree of maturity during the whole of life, from the first month after birth till about the age of 40. 2. The greater part of the follicles are not ripe, do not burst, and do not discharge their contents, but undergo atresia, presenting an almost complete analogy with that of the formation of the corpora lutea. 3. The development and maturation of the Graafian follicles are not produced periodically in a regular manner, and no connection exists between them and menstruation. 4. Menstruation constitutes a physiological phenomenon, quite independent of the development and maturation of the follicles. 5. The rupture of follicles more or less mature always bears a certain relation to congestions of the genital organs, produced by any cause whatever. 6. There exist certain maladies (ague, poisonings, &c.) which produce atresia of the follicles at different periods of their developments, after a parenchymatous inflammation of the ovary."

The Termination of Nerves in the Lips.—Dr. Pallidino (*Bull. dell' Assoc. dei Natural di Napoli*) states that in the lips of the horse, which are richly supplied with nerves, many isolated, non-medullated fibres run from the subcutaneous connective tissue into the deeper layers of the epithelium, when they have a straight course and terminate by free extremities after they have traversed the deepest layer of the pavement epithelium, occasionally exhibiting a terminal dilatation or enlargement. Pallidino has not been able to discover any connection of the nerve fibres with peculiar stellate cells of the rete Malpighii, as described a year or two ago by Langerhaus.

Distinction between Mammalian and Reptilian Blood.—The 'American Journal of Medical Sciences' says that Dr. R. M. Bertolet, M.D., Microscopist to the Philadelphia Hospital, refers to the great difficulty

which is experienced in determining the kind of blood, by the ordinary methods of examination in medico-legal cases.

If examined with the microscope, as it is ordinarily found in the dried state, the corpuscles are shrivelled and deformed. The addition of water extracts the colouring matter, and though it causes them to swell up, does not restore them to their original condition. It causes the red corpuscles to lose their bi-concave shape and approach the spherical. The oval disks of reptiles, birds, &c., lose something of their peculiar shape, and become more like mammalian blood.

In moistening such blood he uses a solution of sulphate of soda, or, better still, slightly acidulated pure glycerine. This preparation "is carefully irrigated with a properly prepared alcoholic solution of guaiacum resin: then, when a very small quantity of the ethereal solution of the peroxide of hydrogen (ozonic ether) is introduced beneath the glass cover," the red corpuscles are changed to an uniform colour, which varies in the different corpuscles, "from a light sapphire to a deep indigo blue."

In the nucleated corpuscles of birds, reptiles, &c., however, "*the nucleus is seen as a sharply-defined, dark blue body, while the protoplasm surrounding it assumes a more delicate violet hue.*" The distinction between the two kinds of blood, by this means, is so plain as to be evident even to an ordinary gentleman of the jury.

What Pus is not.—The following interesting paper is contributed to the 'Medical Examiner' (Chicago, U.S.A.) for April, by Dr. Lester Curtis, M.D. :—

"A few years ago Conheim published some observations on the white blood corpuscle, which confirmed the older observations of Waller and Beale, and called attention to them; for previous to this time they had attracted little notice, especially on the continent of Europe. These observations showed that, in inflammation, many of the white blood corpuscles pass through the walls of the capillaries, and appear outside of them. The corpuscles outside the vessels continue their amœbiform movements, and possessing the power of locomotion, were called 'wandering *cells*.' (?)

"At the time of these observations it was well known that the fresh pus corpuscle also had an amœbiform movement similar to that of the white blood corpuscle. Pus occurs as the result of inflammation; and where there is inflammation there are large numbers of wandering cells. Conheim concluded, therefore, that pus corpuscles came from the wandering cells, and, as the wandering cells came from the white blood corpuscles, therefore that a pus corpuscle was a white blood corpuscle. He rejected as erroneous the previous opinion that pus could be derived from any other source than the white blood corpuscles.

"Conheim's conclusion, that the pus corpuscle and the white blood corpuscle are identical, has been widely accepted. It is due partly to the acceptation of this theory that the name '*leucocyte*' has arisen—a name which is applied indiscriminately to the white blood corpuscle, the lymph corpuscle, the wandering cell, and the pus corpuscle. Some, in publishing their acceptation of the theory, have added the

saving epithet '*morphologically*' to the '*identical*,' evidently implying some doubt, after all, as to its correctness.

"In spite, however, of the general acceptation of the opinion, it appears to me to be inconsistent with certain well-known facts. It is my purpose to present some of these facts, and show wherein they are inconsistent with the theory. I shall consider the subject from Conheim's standpoint : supposing that all pus originates from white blood corpuscles, although I consider the proof of such sole origin as far from complete.

"In the first place, it by no means follows that, because a pus corpuscle is derived from a white blood corpuscle, it is identical with a white blood corpuscle. The white blood corpuscles are mere stages of growth, just as a chrysalis, or a tadpole, is a stage of growth. They have no particular function of their own, as, for instance, the red corpuscles have; they only exist in order that they may be developed into something else. If this is the case, it is not only supposable that, under the changed conditions of nutrition to which the wandering cells are subjected outside the vessels, they should undergo a change; but it is difficult to understand how they should continue to be the same that they were within the vessels.

"Mere similarity of form and appearance is, as we all know, one of the least reliable of resemblances; and the fact that a pus corpuscle appears to be like a white blood corpuscle can surely go but a short way towards establishing their identity. The sporules of fungi can often be crushed, and the softer, central portion can be freed from the envelope. When this is done, the central portion of the sporule may resemble a white blood corpuscle so closely in every particular, except, perhaps, in size, that even an experienced observer would be unable to distinguish them apart. Would anyone, on this account, consider them to be identical? There must be other resemblances between two bodies besides form and appearance merely, to render them identical. They must correspond in all essential particulars; and if they differ in any essential particular, they plainly are not identical. Now let us see if pus corpuscles correspond in all essential particulars with white blood corpuscles.

"The white blood corpuscles of every healthy person correspond in every particular with which we are acquainted, with the white blood corpuscles of every other person; and while there may be, and probably are, points in which the corpuscles of every individual differ from those of every other individual, these differences are so slight that the corpuscles of one person may be substituted for those of another, by transfusion of blood, without disturbance of function. If, then, pus corpuscles are the same thing as white blood corpuscles, all pus which has not a specific origin should be similar. I need hardly say, however, that this is notably not the case. No one would suppose for an instant that the pus from an ordinary abscess, and that from a purulent ophthalmia were the same. Yet the bland and unirritating pus from the abscess, and the highly contagious pus from the purulent ophthalmia, may have had their origin in a simple, and perhaps similar irritation; and the white blood corpuscles of the

two individuals may preserve their similarity at the same time that the pus shows such great differences. Can things which differ from each other both be similar to the same thing?

"Again, the physiological action of pus differs from that of a white blood corpuscle. White blood corpuscles may easily, and with safety, be transferred from the vessels of one individual to those of another; but if pus is injected into the vessels, the result is a serious disturbance. The experiment has been tried of injecting pus into the veins of an animal; a febrile action, dangerous to the life of the animal, is the result; and if some of the blood of this animal is injected into the veins of a second animal, a still severer disturbance than in the first animal is set up. If the blood of the second is injected into the veins of a third, a similar disturbance is set up; and so of a fourth, and so on. The introduction of pus into the veins of the animal has given rise to profound changes in its blood—an effect differing widely from the harmless result of the introduction of the blood corpuscle.

"Again, the white blood corpuscles can become organized, and form tissue; or, at least, the wandering cells outside the vessels can become organized; and it is a well-known fact, that from these wandering cells all inflammatory new formations arise. Some, indeed, maintain that from such wandering cells are produced all the new growth of connective tissue, and all the new formations in the body. Pus, however, cannot become organized, as anyone who has observed the mischief done by a small quantity of pus beneath the periosteum of a finger can well appreciate.

"If pus, then, originated from a white blood corpuscle, it has lost the power of organizing; and who can tell how great is the difference which has resulted from that loss?

"Again, if the pus from our purulent ophthalmia, which may have arisen from a simple irritation, be introduced beneath the lid of a well person, it will, in all probability, set up a disease similar to that in the eye from which it was taken. If a white blood corpuscle had the property of setting up disease, what surgeon would be skilful enough to avoid purulent ophthalmia? The pus from purulent ophthalmia, then, has not only lost the power of organizing, but has acquired noxious properties, which render it hurtful to the person in whom it originated, and dangerous to those with whom it may come in contact. Can any two things differ more widely than the blood corpuscle and this pus—the one a useful and necessary part of the body, and the other a breeder of disease, and an object to be dreaded?

"In what I have said, granting what I do not believe, that all pus originates from white blood corpuscles, I have tried to show:—

"1st. That white blood corpuscles, being in a transition stage, we have no right to expect that, in the changed condition of nutrition to which they are subjected, outside the vessels, they would continue to be the same that they were within the vessels.

"2nd. That mere similarity of appearance was insufficient evidence of identity.

"3rd. That different samples of pus are unlike each other; which they would not be if they were white blood corpuscles.

"4th. That pus differs from white blood corpuscles.

"*a.*—In the disturbance which it sets up when introduced in these vessels.

"*b.*—In the loss of the power of organizing.

"*c.*—In the frequent acquisition of contagious properties.

"These are some, though by no means all, the reasons why I consider that pus is not the same thing as a white blood corpuscle. If I have established the point, it will be something gained; if I have failed, I would esteem it a favour to be shown my error."

Structure of Bœhmeria nivea.—The structure of the aërial stem of *Bœhmeria nivea*, a plant belonging to the nettle family, yielding the well-known China grass or Rhea fibre, was described by Mr. H. Pocklington, at a late meeting of the Leeds Naturalists' Field Club, as follows:—The central pith is peculiarly white and glistening to the naked eye. This is doubtless due to the excessive tenuity of the walls of the cells composing the medulla, to their being devoid of all proteinaceous contents, and to their inclusion of nothing but air when in the dry state. Most of the light incident upon them when viewed *in situ* will be totally reflected from the surfaces of the air within the cells, and thus give them the appearance of being illuminated by a clear lunar light from within. The medullary sheath is well developed, and consists, excluding the ordinary woody fibre, of large triple-spiral vessels, boldly barred *bothrenchyma* and long cylindrical cells containing a yellowish fluid soluble in alcohol. The fibre of the spiral vessels is strong, and easily separates from the primal wall of the cell, and the "barred" vessels are somewhat remarkable for their coarseness when contrasted with the vessels of the woody zone. The yellowish oil has not been investigated as yet, but appears to be a chlorophylloid product. The woody zone is well developed, and is remarkable for the nature of the cells of which it is composed. The normal spindle-shaped inactive much-thickened wood fibres are here replaced by thin-walled prosenchymatous cells containing, beside proteinaceous matter, large quantities of starch granules, and by less obviously wood-cells, minutely porous and also containing starch. Starch-bearing wood-cells have been described by Hassall* and myself† as occurring in certain roots and rhizomes, but they have not, so far as I know, been hitherto described as occurring in aërial stems. The occurrence of them in roots is entirely unnoticed in our text-books, and is unknown to many botanists of extensive knowledge. The medullary rays are not evident in transverse section, but may be easily recognized in longitudinal sections. They are very much longer than broad, sometimes thickened, and contain little beside sap and starch. The bothrenchyma is interesting. The pits are oval, sometimes complete pores, and in the centre of a discoid, rhomboidal, or polygonal ternary deposit, with an irregular spiral of secondary deposit running between them. These are in fact very good examples of what are known as bordered pits, but must not be confused with the glandular pleurenchyma of conifers. The starch granules are varied in shape. The

* 'Adulteration Detected.' † 'Pharmaceutical Journal,' 1872–3.

larger number are round or ovoid, some are semi-mussel shaped, a few almost bacilliform; many are compounds of two, most are single granules. All give a black cross with considerable distinctness by polarized light. The cortical layers are chiefly remarkable for the liber fibres which constitute the China grass of commerce, and the small sphæraphides that accompany these linearly. The liber cells are, as shown long since by Quekett, very much stouter than those of flax, and are easily to be distinguished from them by means of a power of 300 or 400 diameters, the transverse markings in the two fibres being very different. The China grass fibres are very tough, their walls are considerably thickened, but they have a large central cavity filled with a mixture of gummy and proteinaceous matter. The result of this is that when the fibres are exposed to moisture after being dried then the contents absorb moisture, the fibres expand laterally and contract longitudinally, so that if they be woven into a fabric the chances are the fabric puckers in a very disagreeable fashion. This is certain to be the case if the fibres be mixed with wool as in certain Bradford manufactures. China grass fibres, however, will doubtless come into use provided a machine can be invented by which they can be economically removed from the hard woody stem. This latter will probably be utilized in the paper manufacture, and some mechanico-chemical means that will preserve the fibres uninjured whilst preparing the pleurenchyma for the paper-maker will probably be discovered one of these days. The other cortical cells do not require any notice. Their contents are chiefly what Mr. Sorby calls endochrome, granular matters of uncertain composition, and the small sphæraphides already referred to. These latter are almost certainly an impure oxalate of lime. The endochrome chiefly consists of yellow xanthophyll. Blue chlorophyll and, probably, small quantities of lichno-xanthine, passing by deoxidation into a pinkish-brown chromule, colouring the bark cells.

The Etiology of Madura-foot.—The 'Indian Medical Gazette' says it has recently received a pamphlet on this subject from Dr. H. Vandyke Carter,* but after careful study of its contents has not been able to alter its opinion in the slightest degree. "This pamphlet and its accompanying plate may, we presume, be taken as an epitome of the author's previous writings and drawings in connection with this malady, doubtless embodying also the experience gained during the dozen years or so which have transpired since his views were first placed before the profession.

"These views are so well known that it is scarcely necessary to refer to them at any great length. Suffice it to say that Dr. Carter believes that he has shown that the disease is caused by a distinct fungus—a peculiar red mould, which has not been seen except in connection with Madura-foot. This mould was first observed by Dr. Vandyke Carter in May, 1861, 'upon part of a diseased foot which had been placed in water for maceration. . . . The next occasion of its

* "The Parasitic Fungus of Mycetoma." By H. Vandyke Carter, M.D.—'Transactions, Pathological Society of London.' 1872–3.

occurrence was during the following year, in the month of April, in connection with a specimen of mycetoma preserved in spirits, and again, also about the same date, the mould was seen on some rice paste in which some fresh black fungus particles had been placed in order to ascertain if they could be made to grow artificially.'

"It will be observed that the mould referred to as having developed under these varying conditions was identified as one and the same kind of fungus—a fact which *per se* contains a sufficient refutation of the whole theory; for it is a physical impossibility that spores of fungi which had been preserved in spirits should retain their vitality, consequently the mould which grew on the spirit-preserved specimen *must have been of extraneous origin;* not only having germinated after the evaporation of the alcohol, but which must have originated from some source other than the interstices of the macerated tissue. We are therefore compelled to infer that the red mould, of various shades, described as having spread over portions of these three and other Madura-foot specimens, was but some developmental form of our ordinary pink-tinted moulds, bearing no relation whatever to the black, yellow, or orange-coloured particles frequently found in diseased tissues of this nature—no closer relationship, in fact, than a crop of various tinted mould on the surface of rice paste does to any coloured particles which may chance to be in its substance.

"No mould with which we are acquainted, however, presents the slightest resemblance to the pink-coloured objects figured in the plate, purporting to represent 'the structure of the red mould found in connection with mycetoma (*Chionyphe Carteri*)'—figures, by the way, differing materially from those appended to the original text in the 'Bombay Transactions,' or any others which we have seen elsewhere, and which, we presume, must be considered as representing the *Chionyphe Carteri* more accurately than the early figures. So long as the forms here delineated are associated in the mind with the idea of *moulds*, one is certainly puzzled to account for their presence; fortunately, however, a sentence in the descriptive text, attached to the plate, supplies us with a key: the objects depicted are referred to as representing 'a fragment of the new growth as this appeared upon a specimen of the foot-disease placed in water to macerate,' and a very good representation it is of 'fragments' which may very frequently be obtained in some specimens of tank water in which, however, no diseased foot need necessarily have been macerated.

"Looking at the drawing, without reference to the text, we should describe the objects as being, probably, some confervoid growths, and the 'spore capsule,' filled with pink-coloured globules, as the encysted gonidium of some Alga, not very unlike the gonidia of Pandorina, as figured in late editions of the 'Micrographic Dictionary,' or Pritchard's 'Infusoria.' To the Alga articles and plates of either of these volumes, or, better still, to some neighbouring tank at certain seasons of the year, we refer our readers for further explanation concerning the objects figured in this plate.

"It is with much regret that we write in this manner concerning any of the labours of so industrious and accomplished an observer as

Dr. Carter is known to be; but when we find a doctrine, which we believe to be altogether erroneous—the result of a misinterpretation of microscopic appearances—used by men of eminence (who themselves may not have the opportunity or possess the special training necessary for this particular branch of study) as a basis upon which to found the etiology of other diseases, we feel that the time has arrived for giving free expression to our opinion regarding it."

On the Smallpox of Sheep.—Dr. E. Klein, Assistant Professor at the Laboratory of the Brown Institution, in a paper read before the Royal Society in June, 1874, says that Variola ovina, or smallpox of sheep, is a disease which, although it is not communicable to man, and possesses a specific contagium of its own, very closely resembles human smallpox, both as regards the development of the morbid process and the anatomical lesions which accompany it. This correspondence is so complete, that it cannot be doubted that the pathogeny of the two diseases is the same. The present investigation was therefore undertaken in the confidence that the application of the experimental method to the investigation of the ovine disease would not only yield results of value, as contributory to our knowledge of the infective process in general, but would throw special light on the pathology of smallpox.

The paper consists of four sections. In the first, the author gives an account of his experimental method, which consisted in communicating the disease by inoculation to a sufficient number of sheep, and in investigating anatomically (1) the pustules produced at the seat of inoculation, and (2) those constituting the general eruption. The lymph employed was obtained by the kindness of Prof. Chauveau, of Lyons, and Prof. Cohn, of Breslau.

In the second section, the organisms contained in fresh lymph, and the organic forms derived from them by cultivation, are described. The author finds that fresh lymph contains spheroidal bodies of extreme minuteness, which correspond to the micrococcus of Hallier and to the spheroids described by Cohn and Sanderson in vaccine lymph. It also contains other forms, not previously described, which in their development are in organic continuity with the micrococci.

The third section contains a complete anatomical description of the skin of the sheep with special reference to those particulars in which it differs from that of man.

The remainder of the paper is occupied with the investigation of the changes which occur in the integument at the seat of the inoculation, and with the anatomical characters of the secondary pustules.

The most important results are the following:—

1. The development of the primary pock may be divided into three stages, of which the first is characterized by progressive thickening of the integument over a rapidly increasing but well-defined area; the second, by the formation of vesicular cavities containing clear liquid (the "cells" of older authors) in the rete Malpighii; the third, by the impletion of these cavities with pus corpuscles and other structures. It is to be noted that the division into stages is less marked than in human smallpox.

2. The process commences in the rete Malpighii and in the sub-

jacent papillary layer of the corium; in the former, by the enlargement and increased distinctness of outline of the cells, and by corresponding germinative changes in their nuclei; in the latter, by the increase of size of the papillæ, and by germination of the epithelial elements of the capillary blood-vessels.

3. It is next seen that the interfascicular channels (lymphatic canaliculi) of the corium are dilated and more distinct; that the lining cells of these channels are enlarged and more easily recognized than in the natural state; and that, in the more vascular parts of the corium, the channels are more or less filled with migratory, or lymph, corpuscles. At the same time, the lymphatic vessels, of which the canaliculi are tributaries, can be readily traced, in consequence of their being distended with a material which resembles coagulated plasma.

4. About the third day after the appearance of the pock, the contents of the dilated lymphatics begin to exhibit characters which are not met with in ordinary exudative processes. These consist in the appearance, in the granular material already mentioned, of organized bodies, which neither belong to the tissue nor are referable to any anatomical type—viz. of spheroidal, or ovoid, bodies having the characters of micrococci and of branched filaments. These last may be either sufficiently sparse to be easily distinguished from each other, or closely interlaced so as to form a felt-like mass.

5. The process, thus commenced, makes rapid progress. After one or two days, the greater number of the lymphatics of the affected part of the corium become filled with the vegetation above described; and on careful examination of the masses, it is seen that they present the characters of a mycelium, from which necklace-like terminal filaments spring, each of which breaks off, at its free end, into conidia. In most of the filaments, a jointed structure can be made out, and, in the larger ones, the contents can be distinguished from the enclosing membrane by their yellowish-green colour.

6. At the same time that these appearances present themselves in the corium, those changes are beginning in the now much thickened rete Malpighii which are preparatory to the formation of the vesicular cavities already mentioned. By a process which the author designates horny transformation, having its seat in the epithelial cells of the middle layer of the rete Malpighii, a horny expansion, or stratum, appears, lying in a plane parallel to the surface, by which the rete Malpighii is divided into two parts, of which one is more superficial, the other deeper than the horny layer. Simultaneously with the formation of the horny layer the cells of the rete nearest the surface of the corium undergo very active germination, in consequence of which the interpapillary processes not only enlarge, but intrude in an irregular manner into the subjacent corium. At the same time, the cells immediately below the horny stratum begin to take part in the formation of the vesicular cavities, some of them enlarging into vesicles, while others become flattened and scaly, so as to form the septa by which the vesicular cavities are separated from each other.

7. The vesicles, once formed, increase in form and number. Originally separate, and containing only clear liquid, they coalesce,

as they get larger, into irregular sinuses, and are then seen to contain masses of vegetation similar to those which have been already described in the lymphatic system of the corium—with this difference, that the filaments of which the masses are composed are of such extreme tenuity, and the conidia are so small and numerous, that the whole possesses the characters of zooglœa rather than of mycelium. However, the author has no doubt that these aggregations are produced in the same way as the others, viz. by the detachment of conidia from the ends of filaments. In the earlier stages of the process the cavities contain scarcely any young cells. Sooner or later, however, so much of the rete Malpighii as lies between the horny stratum and the papillæ becomes infiltrated with migratory lymph-corpuscles. The process can be plainly traced in the sections. At the period of vesiculation, i. e. at a time corresponding to the commencement of the development of the vesicles in the rete Malpighii, the cutis (particularly towards the periphery of the pock) is infiltrated with these bodies. No sooner has the coalescence of the vesicles made such progress as to give rise to the formation of a system of intercommunicating sinuses, than it is seen that the whole of the deep layers of the rete Malpighii become inundated (so to speak) with migratory cells, which soon find their way towards the cavities, and convert them into microscopical collections of pus corpuscles, the formation of which is proved to be due to migration from the corium, not only by the actual observation of numerous amœboid cells *in transitu*, but by the fact that the corium itself, before so crowded with these bodies, becomes as the pustulation advances entirely free from them.

8. The concluding section of the paper is occupied with the description of the secondary eruption, the anatomical characters of which closely resemble those already detailed.

On the Morbid Anatomy of Progressive Muscular Atrophy.—In a very valuable pathological contribution,* Dr. Lockhart Clarke has described the microscopical appearances observed in a case of muscular atrophy, accompanied by muscular rigidity and contraction of the joints. The parts received for examination were a slice of one of the cerebral hemispheres, the cerebellum, pons Varolii, medulla oblongata, and spinal cord. The white substance of the brain was rather thickly interspersed with corpora amylacea, from about twice the diameter of a blood disk to fourteen times that size. In the grey substance only a few of these bodies were present, and they were confined chiefly to the deeper layers. These are thus detailed by Mr. W. B. Kesteven in the 'Medical Record,' June 24th:—

It is here worthy of note that in chronic disease of the brain and spinal cord the presence of bodies, of varying size and far from uniform aspect, to which the name of amyloid bodies is generally given, is by no means uncommon. At the same time there are forms of degeneration of the neuroglia which give rise to appearances so closely resembling the so-called corpora amylacea that it is an extremely difficult thing to distinguish between them. Minute spots of miliary sclerosis,

* 'Medico-Chirurgical Transactions,' vol. lvi. 1873.

and of colloid, are often to be seen in the same sections with the supposed amyloid bodies. The chromic acid, or other means employed to harden the nerve substance, so far alters its condition that the reactions of iodine or other tests for cellulose are controlled or obscured.

Dr. Clarke notes a dilated condition of the vessels, and in some parts a disintegration of these to the extent of causing their entire disappearance, with a consequent production of large, empty, and smooth-walled tubular spaces, which, according as they were cut transversely or obliquely, presented an appearance of round or oval vacuities. This appearance was first described by the author in a case of general paralysis of the insane,* and has since been noticed also by other observers. It formed the most prominent feature of the lesions described by Dr. Dickinson in the medulla oblongata from several cases of diabetes. Dr. Clarke also refers to the dilated condition of the vessels, in connection with those spaces around them which have been spoken of as "lymphatic spaces," or "perivascular sheaths"; but which, the reporter has endeavoured to show, are the results of pathological, or even of merely *post mortem* changes.

The cells of the cerebral grey substances in this case were not altogether healthy. Some of them had lost their natural sharpness of outline; others contained rather more pigment than usual, or were somewhat granular at their surfaces. The pigmentation of cells was still more observable in the medulla oblongata. This change is considered by Dr. Clarke to constitute the first stage in the degeneration and subsequent disintegration of nerve cells. The medulla oblongata was one-fifth below the average size, and the diameter of the spinal cord was reduced by at least one-fourth; so much was it reduced that when first seen by Dr. Clarke, without any explanation, he thought it was the cord of a child of fourteen years of age.

The grey matter of the cord presented a variety of lesions. Congestion of the white columns was present. Hypertrophy of the connective tissue, with proliferation of its corpuscles, and aggregation of these in masses at the angles of junction in the network, are described by the author, and illustrated in an engraving. Several patches of disintegration were observed. One of large size consisted of small remnants of partly disintegrated grey substance, irregularly connected with each other, and forming together a kind of reticular or honeycomb structure. Several large areas of disintegration and hæmorrhagic clots existed, involving large portions of the cord in destruction. In all regions of the cord, the nerve cells had undergone degeneration and disintegration. Some were completely, others only partially, filled with dark-brown pigment granules, which in many instances enveloped and concealed their nuclei. All the remaining cells were reduced in size; many seemed to have been lost by gradual atrophy, and numbers had wholly disappeared by complete disintegration, or fallen into granules. The several stages of the process could be followed.

We have very imperfectly followed Dr. Clarke in the details of

* 'Journal of Mental Science,' January, 1870.

the changes he records. They are well and clearly shown in the drawings by which the paper is accompanied. As the author remarks: "The symptoms in this case are very clearly explained by the morbid changes that were formed in the medulla oblongata and spinal cord. Lesions were traced in the nuclei of the facial, hypoglossal, vagus, and spinal accessory nerves, and explained the symptoms of glosso-pharyngeal paralysis. The extensive loss of substance in the anterior and lateral grey substance of the cervical and dorsal regions, more especially of the *tractus intermedio-lateralis*, explained feebleness of respiratory movements, while progressive changes of similar character in the lumbar and dorsal regions of course explained the paralysis of the upper and lower extremities."

Bone-Absorption by means of Giant-Cells.—Mr. Alexander Morison,* taking up the researches of Kölliker on absorption of bone by means of giant-cells, finds, says Mr. Klein, in 'Medical Record,' July 8th, 1874, on examination of sections through the jaw prior to the formation of the tooth-sac, that many giant-cells contain clear round or oval holes of various sizes. The larger and more distinctly defined ones, in the centre of which a débris resembling fatty particles is sometimes to be detected, appear to be originated by a disintegration of minute portions of the protoplasm of the giant-cell. From this the author takes it as possible that the giant-cells, after having ceased to exercise their destructive, i. e. absorbing function, become disintegrated. Morison takes it also as probable that sequestra are separated from living bone by means of giant-cells, for, on examining a fresh sequestrum, from a case of necrosis of the tibia, there were found Howship's lacunæ covering all aspects of the sequestrum, and the blood and pus around the preparation containing multinuclear giant-cells floating about.

As regards the origin of giant-cells, Morison agrees with Kölliker and others that many of them are in genetical connection with the osteoblasts, but that others probably develop from embryonic connective tissue; for there occur bone spaces with here and there a giant-cell entirely destitute of osteoblasts, but containing the nuclei of embryonic connective tissue. These nuclei, generally scattered, are here and there closely aggregated and show an internuclear opacity, which, however, has not the distinctly granular appearance of the opaque cell-substance of a fully developed giant-cell; but this appearance is in variable degree, even in fully formed cells. It is possible that the aggregation of nuclei may be the first stage in the formation of a giant-cell; one has only to imagine that these nuclei prepare a cell material each around itself, which, coalescing with that round its neighbours, produces the multinuclear giant-cell.

Morphology of the Saprolegniei.—The 'American Naturalist,' June, 1874, says that this doubtful family, that seems now finally deposited in the Algæ, has considerable economic interest from the destructive effects produced upon fish eggs in the hatching trays, supposed to be caused by *Achlya prolifera.* The following summary is translated

* 'Edinburgh Medical Journal' for October, 1873.

from advance sheets of "Contributions to the Morphology and Systematic Relations of the Saprolegniei," by N. Pringsheim.*

The results of my investigations on the Saprolegniei may be condensed as follows:—

1. In all the Saprolegniei the male organs of generation develop from the well-known antheridia, that are formed near or grow toward the oogonia.

2. Those in which antheridia or their equivalents are wanting, are not, as has been supposed, distinct species, with modified organs, but parthenogenetic forms, whose sporangia ripen and bud without fertilization.

3. In the Saprolegniei there is but one kind of sporangia; those which develop parthenogenetically, and those which are fertilized are identical, and show no difference originally. The unfertilized zoospores grow sooner and more readily than those which are fertilized.

4. Several peculiarities in the formation of zoospores, which have been considered sufficient specific distinctions, are not important as such, but are merely evidences of a greater or less tendency to dimorphism, representing various stages of development in the zoospores.

5. Also various sexual forms of growth may appear in the same species, which are not reliable as specific distinctions.

The Histology of the Brain in the Insane.—Very many physicians who have given attention to this subject are of opinion that the structure of the brain is not materially, if at all, altered in disease. Now, however, a different view is expressed in a paper read before the Chicago Society of Physicians and Surgeons, and reported in the 'Medical Examiner' (a Chicago paper) for June 15. The paper in question was prepared by Dr. Walter Kempster, of the Northern Asylum for the Insane, at Oshkosh, Wisconsin, formerly of the New York State Lunatic Asylum, at Utica, and he had made microscopical examinations in forty-nine cases. Numerous slides were exhibited of sections, made mostly through the third left anterior cerebral convolution, illustrating the lesions of acute mania; the large sclerous patches in chronic mania; the dementia of syphilitic paralysis; one section through the olivary body, and one through the pons Varolii—each illustrative of acute mania.

Numerous micro-photographs were likewise shown, illustrating the lesions of cerebro-spinal meningitis; of numerous colloid masses in the medulla oblongata, and large degenerated masses with dense fibrous investing membrane in the spinal cord, opposite second cervical vertebra—each illustrative of acute mania. Also, a section through the olivary bodies, in a case of puerperal mania, showing fibres and connective tissue in degenerated masses.

After acknowledging the great abilities and researches of Lockhart Clarke, Virchow, Meynert, Schultze, Deiters, and others, in the study of the nervous system, Dr. Kempster remarks that, so far as he is aware, none of them have directed especial attention to the abnormalities found in the brains of those who die while insane.

* 'Jahrbuch für wissenschaftlicher Botanik,' ix, Bd. 2tr. Heft.

Reference was made to an article in the 'Edinburgh Medical Journal' for September, 1868, by Dr. J. B. Tuke, as being the only exception which Dr. Kempster was able to find.

The student is met with the stereotyped phrase that there are no discernible lesions peculiar to insanity. For a number of years Dr. Kempster has been making systematic microscopical study of the brain, and has examined the lesions of all forms of insanity, from acute mania to dementia, including puerperal and epileptic insanity.

In each and all forms he has found a marked lesion—so that certain lesions may be grouped together as common to certain forms of insanity, and to which lesions any particular type of insanity is palpably due. There is a wide difference between the lesions of acute and chronic mania.

I. In certain forms of insanity, and notably in dementia, the finer capillaries show marked indications of disease, the perivascular sheath surrounding the vessel is distended, so much so, that sometimes the vessel itself appears to lay in a tunnel, its calibre being much less than the sheath, doubtless due to repeated capillary congestions of the vessels often diseased—irregular in calibre, suggesting the idea of aneurismal dilatations, but entirely distinct from the miliary aneurisms so ably described by Charcot.

II. Next, there is a degeneration, best studied in cases of dementia of syphilitic origin, and in the medulla oblongata, in the wall of the capillary, presenting dark red patches at various points outside its walls, which gradually thicken, and appear to be due to a fatty metamorphosis or atheroma. The description by Meynert, though accurate, is by no means so complete as could be desired.

III. In 1871, while examining a section taken from the grey and white matter of the third left anterior convolution, there was a peculiar appearance of the tissue. Situated in the white substance, but very closely to the grey matter, there were a number of small *white spots*, some round, some ovoid, clearly defined, in sharp contrast with the nerve tissue, varying in size, from 1–50 to 1–200 of an inch in diameter—these appeared to be of a granular consistence, and much more dense in structure than the surrounding brain substance; each disconnected from the other, and normal white matter intervening. They did not absorb carmine, and were not connected with the capillaries. On the surface of some of the spots are fibres of connective tissue and crystals of margarine. To determine the true character of these spots and the degeneration, certain very elaborate and extensive micro-chemical manipulations were made, not here necessary to be stated. On allowing a section to dry, either with or without the nitric acid treatment, these spots appear to project above the surface of the section. By teasing, they may with difficulty be removed. None of these spots have been observed in the grey matter. They are most numerous in the medulla oblongata, and may be found in the white matter of the spinal cord.

IV. There is another form of degeneracy, one which was found in cases of acute mania. The spots are less in size; are far more numerous than in the other variety (3); resist carmine staining; do not

possess the granular characteristic; there are no spindle-shaped fibres of connective tissues about them; they behave very differently under the micro-chemical tests applied to the other variety of spots. The points of resemblance are mainly in colour and apparent density. Neither of them have any investing membrane.

V. A fifth variety, as large in size as the third, possesses a dense investing membrane, which resists carmine staining and is less granular than the third and fourth. It exists in the same brain with the fourth variety. These spots or masses of the fifth variety are called "colloid," because of their resemblance to such growth, and are found in the medulla oblongata and pons Varolii. The last three varieties of degenerated masses, or spots, have one feature in common—a well-defined edge, a clean-cut margin, easily made out.

VI. A sixth variety, common in cases of dementia, and where the atheromatous capillary is found, is one in which the mass passes insensibly into the surrounding normal tissues. This form is larger and less distinct than the others. It more nearly resembles normal brain tissues. Sometimes these masses are lobulated. They are granular and dense, less numerous than in the other varieties, and do not appear in clusters. They appear to destroy or transform the tissues, and if surrounding a capillary, destroy its walls. A point of resemblance in common with the third variety is, that connective-tissue fibre appears in both.

The condition of the cellular structures of the brain, of the nerve fibres and so-called lymph spaces, are all fields rich in results not here spoken of.

The Development of Bone.—Perhaps the first authority on this subject at the present moment is M. Ranvier, who lately read a paper on it before the French Academy of Sciences. This paper forms the subject of the following note, which is communicated by Mr. E. Klein to the 'Medical and Surgical Recorder' for July 15th. To study the growth and development of bone tissue, Ranvier uses the bones of the embryo, which are placed in absolute alcohol for twenty-four hours, having previously been freed of the surrounding soft parts (except the periosteum). After that, they are transferred to a saturated solution of picric acid, in which fluid they are kept until they become soft enough to be fit for sections. In order to make thin and successful sections, the softened bone is plunged into a thick solution of gum-arabic for forty-eight hours, and then into alcohol of forty degrees. Now it is easy to obtain very uniform sections through all parts of the bone, i.e. bone matrix, medulla, and periosteum. The sections having been washed in distilled water for twenty-four hours or more, in order to dissolve the gum, they are stained with picro-carminate of ammonia, and finally mounted in glycerine. In a longitudinal section through a long bone of an embryo of a mammalian animal, passing from the periosteum towards the axis, it is easy to see a well-marked boundary between the periosteal bone and the cartilaginous bone. The latter occupies the centre, and has an hour-glass shape in the longitudinal section, whereas the periosteal bone forms on each side a semilunar mass. The long bone at this stage of development may be correctly

compared to the following scheme: an hour-glass shaped cartilaginous bone is suspended in a cylindrical tube—the periosteum; that part of the space of the tube which is not occupied by the former is filled out by periosteal bone. This arrangement, although not found in all stages, is always present in a certain stage of the development of the bone. If one examine in a longitudinal section above mentioned the line of ossification, which represents at the same time the boundary between the cartilage and bone, there is found at the extremities of that line a notch penetrating into the cartilage. It is very easily understood that this notch represents the transverse section through a circular groove. From the convexity of this notch ("encoche d'ossification"), fibres take their origin, which, at their basis being identical with the matrix of the cartilage, bend round to the side of the embryonal bone and penetrate into the latter. These fibres, which Ranvier calls "fibres arciformes," become in time identical with those fibres known as Sharpey's fibres. Amongst the mammalian animals, the embryonal bones of sheep are best suited for the study of those fibres. As soon as these fibres have left the cartilage, they appear to be separated by rows of spherical or slightly polyhedral cells, which Ranvier believes to be derived from cartilage cells after their capsules have become opened. These cells gradually assume the characters of osteoblasts, and they lie all along the arched fibres, the latter becoming covered with bone substance, and thus representing the first traces of subperiosteal bone. The arched fibres represent the directing fibres of the ossification; they can be recognized in the interior of the bone in transverse sections, where they appear as small dotted circles in the systems of the intermediary lamellæ.

On the external surface of that part of the cartilage belonging to the "encoche d'ossification," a primary osseous lamella is formed, which Ranvier calls the perichondral bone-crust; it forms later on the boundary between the cartilaginous and the periosteal bone.

Variation in the Condition of the External Sense Organs in Fœtal Pigs of the same Litter. Mr. Burt G. Wilder, of Ithaca, N. Y., says that in comparing fœtal mammals of unknown age, it is natural to estimate their relative age, partly according to the degree of closure of the lids and the direction of the pinnæ; since it is known that the former are at first mere folds above and below the uncovered balls, which are gradually covered by them; and that the pinnæ are first formed as little triangular folds behind the meatus, which at first project directly forward, and then, as they increase in size, gradually rise to the erect position, and only later are retroverted upon the neck.

While forming a collection of fœtal pigs at the large *abattoir* of J. P. Squiers in East Cambridge, Mass., during the summer of 1872, I compared the individuals of the same litter, carefully avoiding any artificial displacement of the parts.

In the five pigs of the same litter* having an average length from vertex to anus of ·067, mm., and an average weight of ,017·5 grams,

* Marked 296 to 300 on the Catalogue of Neurology and Embryology of Domesticated Animals at the Museum of Comparative Zoology, Cambridge, Mass.

the direction of the pinna ranges from a slight but decided *anteversion*, to an almost complete *retroversion*.

In the seven pigs of another litter* averaging ·040, in length, the lids range from folds covering slightly the upper and lower margins of the ball, to complete closure. The sizes and degrees of closure do not exactly coincide. It would be interesting in both these cases to know the relative position of the individuals in the mother's uterine cornua; but these facts indicate the need of far more extended comparisons than have been made.

I have also observed some striking changes in the form of the nostril in fœtal pigs; it is in its earliest condition a notch, whose lower margins then come together forming a hole; this elongates laterally and is indented above so as to become more and more crescentic; but at or before birth the circular form is regained and retained through life.

To what Group is Peripatus related?—In the very last number of the 'Proceedings of the Royal Society' is an admirable paper on this subject by Mr. H. N. Moseley, M.A., of the 'Challenger' expedition. Mr. Moseley enters into details concerning certain points in anatomy which appear to have been wrongly or imperfectly described before. Thus he describes fully the Intestinal, Tracheal, and Reproductive systems, and gives an outlinear sketch of the development. Then he goes on to say that "in the present state of our knowledge concerning the structure of *Peripatus*, the most remarkable fact in its structure is the wide divarication of the ventral nerve cords. The fact was considered remarkable, and dwelt upon in all accounts of *Peripatus* before the existence of tracheæ in the animal was known, and when it was thought to be hermaphrodite, but it is doubly remarkable now. The fact shuts off at once all idea of *Peripatus* being a degenerate Myriopod, the evidence against which possibility is overwhelming. The bilateral symmetry and duplicity of the organs of the body, the absence of striation in the muscles, of periodical moults of the larval skin in development, and of any trace of a primitive three-legged condition, taken in conjunction with the divarication of the nerve cords, are conclusive. The parts of the mouth are not to be regarded as degraded to any great degree; and homologies for some of them, at least, may perhaps be found amongst the higher Annelids. The structure of the skin is not at all unlike that in some worms, especially in its chitinous epidermic layer, which occasionally strips off in large pieces as a thin transparent pellicle. The many points of resemblance of *Peripatus* to Annelids need not be dwelt upon; they led to its former placing in classification; but it is difficult to understand how the very unannelid-like structure of the foot-claws did not lead others, beside De Quatrefages, to draw a line between *Peripatus* and the Annelids. In being unisexual, *Peripatus* is like the higher Annelids, as well as the whole of the higher Tracheata. To Insects *Peripatus* shows affinities in the form of the spermatozoa, and the elaboration, structure, and bilateral symmetry of the generative organs,

* Marked 303 to 309 in the same catalogue.

though there is a very slight tendency towards the unilaterality of Myriopods in the male organs.

"To Insects, again, it is allied by the five-jointing of the feet and oral papillæ and the form and number of its claws. It should be remembered that spiders' feet are two-clawed, as are those of some Tardigrades, and that some of these latter forms have two-clawed feet in the early condition even when they possess more claws in the adult state. In Newport's well-known figure of the young *Iulus* with three pairs of limbs, the tips of these latter are drawn with *two* hair-like claws; these are not mentioned in the text. To the ordinary lepidopterous larva the resemblances of *Peripatus* are striking —as, for example, the gait, the glands (so like in their function and position to silk-glands), the form of the intestine, and the less perfect concentration of the nervous organs, as in larval insects. To Myriopods *Peripatus* is allied by the great variety in number of segments in the various species, in its habits, and in these especially to *Iulus*. The parts of the mouth perhaps show a form out of which those of *Scolopendra* were derived by modification; but the resemblance may be superficial. Our knowledge is not yet sufficient to determine such points. The usual difficulties occur in the matter. Segments may have dropped out or fused, and their original condition may not be represented at all in the process of development. In structure *Peripatus* is more like *Scolopendra* than *Iulus*, viz. in the many joints to the antennæ (in Chilognaths never more than fourteen), in the form of spermatozoa, and in being viviparous, as are some *Scolopendræ*; further, in the position of the orifices of the generative glands and in the less perfect concentration mesially of the nerve cords in *Scolopendra*.

"*Peripatus* thus shows affinities, in some points, to all the main branches of the family tree of Tracheata; but a gulf is fixed between it and them by the divarication of the nerve cords: tending in the same direction are such facts as the non-striation of the muscles, the great power of extension of the body, the arrangement of the digestive tract in the early stage, the persistence of metamorphosis, and the nature of the parts of the mouth, the full history of the manner of origin of these being reserved.

"There are many speculations as to the mode of origin of the tracheæ themselves in the Tracheata. Professor Häckel * follows Gegenbaur, whose opinion is expressed in his 'Grundzüge der vergleichenden Anatomie,' p. 441. Gegenbaur concludes that tracheæ were developed from originally closed tracheal systems, through the intervention of the tracheal gills of primæval aquatic insects now represented as larvæ. If *Peripatus* be as ancient in origin as is here supposed, the condition of the tracheal system in it throws a very different light on the matter. *Peripatus* is the only Tracheate with tracheal stems opening diffusely all over the body. The Protracheata probably had their tracheæ thus diffused, and the separate small systems afterwards became concentrated along especial lines and formed into wide main branching trunks. In some forms the

* 'Biologische Studien,' p. 491.

spiracular openings concentrated towards a more ventral line (*Iulus*); in others they took a more lateral position (Lepidopterous larvæ, &c.). A concentration along two lines of the body, ventral and lateral, has already commenced in *Peripatus*. The original Protracheate being supposed to have had numerous small tracheæ diffused all over its body, the question as to their mode of origin again presents itself. The peculiar form of the tracheal bundles in *Peripatus*, which consists of a number of fine tubes opening into the extremity of a single short common duct leading to the exterior of the body, seems to give a clue. The tracheæ are, very probably, modified cutaneous glands, the homologues of those so abundant all over the body in such forms as *Bipalium* or *Hirudo*. The pumping extension and contraction of the body may well have drawn a very little air, to begin with, into the mouths of the ducts; and this having been found beneficial by the ancestor of the Protracheate, further development is easy to imagine. The exact mode of development of the tracheæ in the present form must be carefully studied; there was no trace of these organs in the most perfect state of *Peripatus* which I obtained.

"Professor Gegenbaur's opinion on the position of *Peripatus** is, that its place among the worms is not certain, but that, at any rate, it connects ringed worms with Arthropods and flat worms. The general result of the present inquiry is to bear out Professor Gegenbaur's opinion; but it points to the connection of the ringed and flat worms, by means of this intermediate step, with three classes only of the Arthropods—the Myriopods, Spiders, and Insects, i. e. the Tracheata. From the primitive condition of the tracheæ in *Iulus*, and the many relations between *Peripatus* and *Scolopendra*, it would seem that the Myriopods may be most nearly allied to *Peripatus*, and form a distinct branch arising from it and not passing through Insects. The early three-legged stage may turn out as of not so much significance as supposed. If these speculations be correct, the Crustacea have a different origin from the Tracheata. *Peripatus* itself may well be placed amongst Professor Häckel's Protracheata; Grube's term, Onychophora, becomes no more significant than De Blainville's Malacopoda. Some notions of the actual history of the origin of *Peripatus* itself may be gathered from its development.

"In conclusion I would beg indulgence for the many defects in this paper, due to the hurry with which it was written (all available time, almost up to the last moment of our sailing for the Antarctic regions, having been consumed in actual examination of the structure of *Peripatus*), and due, further, to the impossibility of referring to original papers in any scientific library. At all events it is hoped that *Peripatus* has been shown to be of very great zoological interest, as lying near one of the main stems of the great zoological family tree, and that further examination of the most minute character into the structure of this animal will be well repaid."

Lesions of the Brain in General Paralysis.—Dr. J. Batty Tuke gives the following instructive account of recent researches on this

* 'Grundzüge der vergleichenden Anatomie,' p. 199.

subject. He says that Lubimoff's paper, published in Virchow's 'Archiv,' vol. lvii., 1873, is founded on fourteen carefully reported cases of general paralysis, which presented themselves in Meynert's 'Psychiatric Clinique.' The full history of each case is given, along with the *post mortem* appearances, naked eye and microscopic. Thin sections were made from specimens hardened in a 2 per cent. solution of bichromate of potass; they were coloured with carmine, and set up in gum Damar. The cortical substance of the frontal lobes was usually examined, and in some cases that of the parietal, occipital, and insular lobes, the cornu Ammonis, and other portions of the encephalon. Lubimoff reports one case in which a sort of cicatrix, or wedge-shaped induration, was found on the right hemisphere of the cerebellum, implicating two lobules which were glued together by a substance which unmistakably consisted of connective tissue. The molecular and nucleated layers were thinned, and Purkinje's cells almost obliterated. For the normal structure a dense "felt-like" substance was substituted, in which nuclei were imbedded, and which was intimately connected with the walls of the blood-vessels. Around it the undestroyed cells of Purkinje appeared plainly sclerosed. Lubimoff supports Meynert's observations as to the intimate relations of brain lesions with hyperæmia, that they never occur apart from it, and may be regarded as a consequence. In some cases the vessels showed indications of obstruction during life by means of thrombi, due to metamorphosis of blood-corpuscles into molecular masses, with here and there distensions filled with corpuscles, and in extreme cases actual rupture of the vascular walls and diffusion of the periphery (*Zerstreuung im Umkreis*) in the parenchyma of the organ. There were also found, in all the fourteen cases, on and around the vascular walls, pigment deposits of various sizes, and sometimes of very considerable extent, which are taken to be evidences of previously existing congestions. Apart from these consequences of hyperæmia, the walls of the vessels presented themselves altered and thickened; their normal coats and muscular striæ being destroyed, and the thickened walls appearing to consist of a homogeneous mass, waxy in appearance. On this Lubimoff bases his term of "waxy degeneration" of the vascular walls. The nuclei, especially at the bifurcations, appeared proliferated. Lubimoff cannot determine whether in general paralysis the vessels thicken themselves by an absolutely new growth.

The special characteristic of paralytic dementia presents itself in the changes of the nuclei of the neuroglia, which show themselves in the brains of such subjects wonderfully increased in quantity, to a degree which, according to Lubimoff, must be accepted as a pathological product, as preparations of healthy brains and of those taken from the subjects of other neuroses (e. g. extreme melancholy and mania), show but a slight amount of neuroglia corpuscles in the cortical substance. (In the opinion of Boll, who has inspected Lubimoff's preparations, this observation is of the highest pathological value).

What Lubimoff describes as nuclei of neuroglia are very fine Deiters' cells, which are well known through the works of Golgi,

Jastrowitz, and Boll; his description is entirely in consonance with that of these writers, and he arrives independently of them at the result, that a peculiar intimate connection exists between the vascular walls and the Deiters' cells, as in these cases their processes are peculiarly well pronounced.

Lubimoff found the Deiters' cells most abundant in the inner layers of the grey matter bordering on the medullary substance, and on the outer layer contiguous to the pia mater; in which position they were so numerous, that the normal appearances of the structures were lost, and their place taken by the felt-like network, which, as in the case of the cerebellum previously described, can only be ascribed to the interlacement in various directions of the processes of the Deiters' cells.

The morbid changes of the nerve cells are placed under two heads; they are liable either to a degree of swelling and subsequent collapse, or to a tendency to sclerosis. In the first case, the changes of the nuclei consist in dilatation of the nucleus and diminution of the quantity of the "surrounding protoplasm"; occasionally the nucleus subdivides so that two are found in one cell, and are not readily amenable to carmine, which Hoffman already has shown to be characteristic of the morbid ganglion-cell. Meynert considers that the protoplasm of such cells shows different degrees of molecular degeneration. The sclerosis of the cells changes them into a homogeneous wax-like mass, in which the nucleus is no longer to be distinguished, but occasionally the nucleolus. The protoplasm of such cells loses its normally fine granular appearance, the cells appear strongly refracting, with sharply defined dark contours. The changes in the axis-cylinders found by Lubimoff consist in thickening and hypertrophy.

It is deduced from these anatomical facts that as regards the pathological processes in general paralysis, the origin of the physical disturbances is to be sought for in the anomalies of blood-distribution and its consequences. With the incidence of hyperæmia begin the changes in the nutrition of the nuclei of neuroglia, which leads to an increased development of their elements, which, in their turn, take on morbid action. This is proved by the modification of the morbid appearances, according to the length of time during which the case has lasted. The treatise concludes with deductions as to how the clinical symptoms of the individual cases are explicable by their special anatomical conditions. Lubimoff agrees with Westphal that disease of the cord is a constant accompaniment of general paralysis; but he differs from him and Simon in holding that the disease can exist without pathological changes in the brain. On the contrary, he endeavours to establish a chronic inflammatory condition of the connective tissue of the cortical substance as the anatomical lesion of general paralysis.—See also 'Medical Record.'

Hay-fever, its Microscopy and Treatment.—This has been very well discussed by Professor Binz of Bonn in a letter recently addressed to 'Nature.' He says, "From what I have observed of recent English publications on the subject of hay-fever, I am led to suppose that

English authorities are inaccurately acquainted with the discovery of Professor Helmholtz, as far back as 1868, of the existence of uncommon low organisms in the nasal secretions in this complaint, and of the possibility of arresting their action by the local employment of quinine. I therefore purpose to republish the letter in which he originally announced these facts to myself, and to add some further observations on this topic. The letter is as follows:*—

" 'I have suffered, as well as I can remember, since the year 1847, from the peculiar catarrh called by the English "hay-fever," the speciality of which consists in its attacking its victim regularly in the hay season (myself between May 20 and the end of June), that it ceases in the cooler weather, but on the other hand quickly reaches a great intensity if the patients expose themselves to heat and sunshine. An extraordinarily voilent sneezing then sets in, and a strongly corrosive thin discharge, with which much epithelium is thrown off. This increases, after a few hours, to a painful inflammation of the mucous membrane and of the outside of the nose, and excites fever with severe headache and great depression, if the patient cannot withdraw himself from the heat and the sunshine. In a cold room, however, these symptoms vanish as quickly as they come on, and there then only remains for a few days a lessened discharge and soreness, as if caused by the loss of epithelium. I remark, by the way, that in all my other years I had very little tendency to catarrh or catching cold, while the hay-fever has never failed during the twenty-one years of which I have spoken, and has never attacked me earlier or later in the year than the times named. The condition is extremely troublesome, and increases, if one is obliged to be much exposed to the sun, to an excessively severe malady.

" 'The curious dependence of the disease on the season of the year suggested to me the thought that organisms might be the origin of the mischief. In examining the secretions I regularly found, in the last five years, certain vibrio-like bodies in it, which at other times I could not observe in my nasal secretion. . . . They are very small, and can only be recognized with the immersion-lens of a very good Hartnack's microscope. It is characteristic of the common isolated single joints that they contain four nuclei in a row, of which two pairs are more closely united. The length of the joints is 0·004 millimètre. Upon the warm objective-stage they move with moderate activity, partly in mere vibration, partly shooting backwards and forwards in the direction of their long axis; in lower temperatures they are very inactive. Occasionally one finds them arranged in rows upon each other, or in branching series. Observed some days in the moist chamber, they vegetated again, and appeared somewhat larger and more conspicuous than immediately after their excretion. It is to be noted that only that kind of secretion contains them which is expelled by violent sneezings; that which drops slowly does not contain any. They stick tenaciously enough in the lower cavities and recesses of the nose.

" 'When I saw your first notice respecting the poisonous action of

* See Virchow's 'Archiv,' vol. xlvi.

quinine upon infusoria, I determined at once to make an experiment with that substance, thinking that these vibrionic bodies, even if they did not cause the whole illness, still could render it much more unpleasant through their movements and the decompositions caused by them. For that reason I made a neutral solution of sulphate of quinine, which did not contain much of the salt (1·800), but still was effective enough, and caused moderate irritation of the mucous membrane of the nose. I then lay flat upon my back, keeping my head very low, and poured with a pipette about four cubic centimètres into both nostrils. Then I turned my head about in order to let the liquid flow in all directions.

"'The desired effect was obtained immediately, and remained for some hours; I could expose myself to the sun without fits of sneezing and the other disagreeable symptoms coming on. It was sufficient to repeat the treatment three times a day, even under the most unfavourable circumstances, in order to keep myself quite free. There were then no such vibrios in the secretion. If I only go out in the evening, it suffices to inject the quinine once a day, just before going. After continuing this treatment for some days the symptoms disappear completely, but if I leave off they return till towards the end of June.

"'My first experiments with quinine date from the summer of 1867; this year (1868) I began at once as soon as the first traces of the illness appeared, and I have thus been able to stop its development completely.'"

NOTES AND MEMORANDA.

Precious Stones in the Construction of the Microscope.—M. H. Brachet addressed a note to the French Academy (June 22nd) on the employment of artificial precious stones in the compound microscope. This paper, which has not yet been published, was sent to the 'Commission du Prix Trémont.'

A Remedy for Phylloxera.—At a meeting of the French Academy of Sciences, June 29th, a paper, forming a Report, was read on the administrative measures to be taken for the preservation of territories threatened by *Phylloxera*, by the Commissioners. It is suggested to the Academy that a special law should be made compelling proprietors to declare the first appearance of the scourge, that experts should then be appointed to examine into the state of the infested vines, and that these should be destroyed when thought necessary by ministerial decision, the proprietor receiving adequate compensation. It is further suggested to destroy the vines surrounding the districts actually invaded, to disinfect the soil by chemical methods, and to burn the cuttings, leaves, and roots of the diseased plants as well as the plants themselves in the same district where the uprooting has taken place, and finally to prohibit with the utmost rigour the exportation

from infested territories of anything that might serve as a vehicle for the insect.

How to make Sections of the Potato showing Structure.—Mr. Taylor has given the following method in 'Science Gossip' for July. He has recently shown that the vascular bundles in a potato may of course be easily seen by cutting a potato in two through its axis (the section also passing through some of its "eyes"), and coating the cut surface, first with a solution of bichromate of potash, and afterwards several times with a strong tincture of iodine, which will stain the starch blue, but leaves the vascular bundles yellow. The air-ducts will then be seen to extend invariably to the eyes. For microscopical study these sections are to be made and treated with a strong acid or caustic alkaline solution, which will dissolve the starch, but leave the bundles unaltered. The sections may then be mounted as usual. To isolate the vascular bundles, place a potato, skinned without wounding the "eyes," in a solution of sugar and water (two ounces to the pint) and keep it at a temperature of 72° F. for nearly a fortnight. The fungus of fermentation will reduce the potato to a pulp, except the vascular bundles, which may be mounted in gum or balsam, and studied with a power of one hundred diameters.

CORRESPONDENCE.

Immersion Aperture on Objects in Balsam.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Further discussion with Mr. Tolles on this question is needless, because by his own work he has settled it to my satisfaction. He has probably secured some believers in his triumph and demonstrations, and to their congratulations I now leave him. So long as imaginary diagrams are used evading the true form of an object-glass, with wrong dimensions and focal distances, futile and endless arguments may be advanced. Including Mr. Tolles' last illustrations in this category, I do not care to be at the trouble of translating his text to arrive at his probable meaning, though I perceive that he is now struggling for *a few degrees only*. He long ago stated that in practice he had actually secured the disputed extra theoretical immersion apertures on balsam-mounted objects, with glasses of his own construction. His recent production having been placed in my hands by Mr. Crisp, I have shown by actual measurement, and by cutting off all false rays by a suitable stop, that such apertures had not been produced, so that "the fact becomes clear and indisputable."

It is very easy for Mr. Tolles to remark that this stop is "a mere contrivance to express my sensations," and "that the whole seems to him quite unnecessary." This I might have expected. But this stop *always is the focal plane of the object*, and admitting all rays up to 180° from that point, will serve as a salutary check against any optician who may choose to vaunt his glasses on the questionable merits of

extravagant apertures.* This point might still give rise to long disputes, but confined to the subject of Mr. Tolles' last communication on "the optical quality of his (Mr. Tolles') $\frac{1}{6}$th objective," there remains the singular fact, that the diameter of the front lens compared with the focal length proves the aperture endorsed on the mount to be *simply impossible.* There can be no reasoning against this but to assert that my dimensions of diameter of front lens, position, and focal distance are untrue. Then I have no doubt that the challenge will be accepted, the measurements repeated, and comparisons made by other hands than mine.

I do not wish to shirk the discussion of the aperture if any further improvement can result from it, but if I am to be at liberty to select my antagonist, it will not be one that affirms that 180° is possible, or that even 179° is practicable.

In justice to Mr. Tolles I will say that throughout all the argument he has comported himself with exemplary good humour, not even resenting the chaff that he has endured during the discussion. A contrast to the conduct of his acting agent, who, without a ray of science to enlighten the subject, or to justify his authority, has done nothing but throw dirt at all opponents, of such an odour, as to cause them to hurry past on his windward side rather than stop to argue with him.

I am, Sir, your obedient servant,

F. H. Wenham.

Reduction of Aperture in Object-glasses of Telescopes.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, U.S.A., *June* 19, 1874.

Sir,—While, substantially, declaring it unlikely that any intelligent reader of the *Monthly* would confound contraction of field in an eye-piece with reduction of aperture in the object-glass (and with which I fully agree, but must suggest the irrelevance of the allusion to beneficial reduction of aperture in *object-glasses of telescopes* † as of some sort of parity to this case of the microscopic objective), Dr. Pigott in concluding his note (your Journal for June, p. 268) says, "that in a limited field of view the definition . . . is superb; beyond the central area the definition is very indistinct. This seems to indicate that the very oblique pencils are not as free from aberration as the central." While thanking Dr. Pigott heartily for having discovered that "very oblique pencils" were *concerned at all* in forming the image with that objective (!) I am bound to suggest that when we contract the field-bar of the eye-piece, the "central area" continues to have all the rays, "very oblique pencils" included, that reached that central area before contraction. This does *not* "seem to indicate," &c.

Yours respectfully,

R. B. Tolles.

* In my diagram, p. 114 of Journal (March, 1874), I had "drawn" ray *d* correctly, but the final deviation without the normal appeared so small on the reduced scale that the wood engraver failed to notice it, and continued the line straight.

† "This everybody knows well enough!"

Mr. Tolles' $\frac{1}{75}$th and $\frac{1}{80}$th Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *June* 23, 1874.

Sir,—The paragraph on p. 264 of the Journal for June, current, and copied from the Boston 'Journal of Chemistry,' in reference to $\frac{1}{75}$ and $\frac{1}{80}$ inch objectives is erroneous, in one particular I *know*, viz. in implying that only one $\frac{1}{80}$th of 165° angle was constructed, whereas three were made simultaneously or carried along together to completion.

In the next place, it is not likely that any comparison with English objectives of like powers or power had been made, and the statement implying such comparison was made under misapprehension.

But reported results of separate trial are available, of course, for comparison.

Yours respectfully,
Robt. B. Tolles.

Dr. Pigott's (?) Inventions.

To the Editor of the 'Monthly Microscopical Journal.'

4, Mylne Street, E.C., *June* 25, 1874.

Sir,—The invention which Dr. R. Pigott claims as his own in the Journal for this month, has been public property since July, 1870. At that time I read a paper on, and exhibited, an apparatus, at a meeting of the Q. M. C., in every respect but one identical with that described by Dr. Pigott. Moreover, I then expressly stated that the plan in question had been foreshadowed by Mr. Sollitt and others.

But I think that I am correct in affirming that no one before myself has ever *publicly* described or used it. The employment of an achromatic objective as a condenser, set *obliquely* to the axis of the microscope, at angles varying with the nature of the object to be examined, is what I claim. But I am of opinion that my method of using it is better than that of Dr. Pigott—inasmuch as I added a graduated circle to the carriage of the objective, in order to measure the angles of use, so as to repeat them exactly in future observations. Dr. Pigott may have privately used this apparatus before my time, but then it was scarcely possible for those who, like myself, had not the advantage of intimacy with him, to be aware of the fact. Moreover, I venture to think that Dr. Pigott does but scanty justice to the labours of others in the same field, when he bestows upon them such faint notice as he has done in my case—even after he had his attention called to them by Mr. Frank Crisp when his paper was read.

I am, Sir, yours obediently,
John Matthews.

Mr. Brooke's Reply to Mr. Pillischer.

To the Editor of the 'Monthly Microscopical Journal.'

16, Fitzroy Square, W., *July* 8, 1874.

Sir,—I should have treated Mr. Pillischer's last letter with the silent contempt that its "schoolboy" tone justly merits, but that my silence might have been taken as an admission of its misstatements.

"Mr. Brooke's argument that native British optical goods were wholly unrepresented at the Vienna Exhibition," is not Mr. Brooke's argument at all, but a pure and simple development of Mr. P.'s inner consciousness. Mr. Brooke's argument was (see May number of Journal, p. 231), that native British optical *talent* was unrepresented; and I think the proverbial "schoolboy" will tell Mr. P. that "goods" and "talent" are by no means synonymous terms.

It is very likely I might have said to Mr. P., "I do not care for your high powers," and the reason of my saying so is obvious; but my recollections of what passed between us are utterly at variance with his. After all, the question is not what A said to B, or B to A, twelve months since, but whether Mr. Pillischer exhibited at Vienna as his own manufacture objectives that were not so. We know very well when, where, and by whom his $\frac{1}{8}$ objectives were made; but if he will satisfy any trustworthy third person (for example yourself, or one of our Secretaries) when, where, and by whose hands he has ever manufactured any power deeper than a $\frac{1}{4}$, I shall be happy to apologise to him for having entertained any doubt on the subject.

I remain, yours faithfully,

Chas. Brooke.

A Reply to Mr. Stodder.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—The purpose of my paper on Immersion is not to establish the superiority of the method, but to determine the cause of it assumed as already established. I am sorry to find it has been taken up at the wrong end, and by the wrong class of persons. As in making my assumption I go chiefly on the testimony of others allowed to be authorities, from myself asserting only a desire to be cautious of overstating, I should have thought that in this there was nothing in which the most diligent seeker could find a pretext for controversy. That whatever its degree the difference is real and to be relied on is all I asked to be granted; an assumption, indeed, necessarily involved, and without which my work would have no meaning. But I cannot go into controversies about what is not my subject. My paper is on a question of Theory, and was meant for those who are competent to follow investigations of that kind.

Mr. Stodder, I observe, has written to charge me with having

fallen into a great error,—an error so great as to be even called ludicrous. When commenting on the angle of a now celebrated glass I referred to it as one which had been sold by Mr. Tolles to an English purchaser. Mr. Stodder writes a special letter to take me to task; for it is he who sends out the glasses not Mr. Tolles. And to have written in ignorance of this makes the writer, in Mr. Stodder's opinion, ludicrous.

Now that the correction has been made it may perhaps be asked how is the question affected by it? How many degrees of the 180° will it account for? And is there any just reason for introducing into the scientific part of a scientific journal a thing which is not scientific or of public interest but only private and commercial? No fault had been found with the sale; nothing was made to turn upon it; no reference was made to its terms about which nothing was known; the question concerned not the terms of the sale but the width of the angle; and the expression used is the common form always employed and never misunderstood. Other opticians do not, any more than Mr. Tolles, make use of their own hands or pens in sending off their glasses; but no distinction is drawn—it is always understood and spoken off as coming to the same thing. An English optician's book-keeper would no more think of writing to explain that it is he who sends off the glasses than of writing to explain how the books are kept. Such details have no interest except for the persons immediately concerned; and no other firm, English or foreign, puts them forward as an element in scientific discussions.

This perhaps was plain enough to require no answer for its own sake. I have noticed it because it is a typical case; illustrative of a practice more common that it ought to be, which may be called frivolous objecting;—that is to say the making of objections which do not in any view of the case or on any supposition touch the question in hand. Such making of objections for the sake of making them is much to be deprecated. It is wearisome and can serve no purpose except obstructing the progress of knowledge.

Yours obediently,

S. L. Brakey.

PROCEEDINGS OF SOCIETIES.

Medical Microscopical Society.

Friday, June 19, 1874.—W. B. Kesteven, Esq., F.R.C.S., Vice-President, in the chair.

Osteo Sarcoma.—Mr. Needham read a paper upon this subject, taking as a foundation the case of a young man who entered one of the metropolitan hospitals with what appeared to be an osteo sarcoma of the head of the tibia. Hard tumours, small in size, were

felt in the groin, as well as deeply beneath the muscles of the thigh. The patient eventually died from chest complication, frequent hæmoptysis being a leading symptom at the *post mortem*. The growth on the leg was found, as diagnosed, to be osteo sarcoma, while similar growths were found on various parts of the body, and especially in the lungs. Microscopically, the growth on the tibia, which was subperiosteal, had the characters of true osteo sarcoma; but elsewhere only calcareous material was found instead of bone with lacunæ.

Specimens and drawings illustrative of the case were exhibited. The case will be published fully elsewhere.

Mr. Golding Bird objected to the term osteo sarcoma, since calcareous deposit in lieu of bone was found except in one place. He considered the earthy deposit as accidental rather than essential to the growth, and as indicating degeneration.

Dr. Pritchard did not consider calcification in all cases a degeneration: from its early appearance at times in morbid tissues, he considered it as much a part of the growth in which it occurred as was true bone in an osteo sarcoma.

Mr. Needham, in reply, agreed with Dr. Pritchard in not considering the calcification as degenerative; and was willing to confine the term osteo sarcoma to the parts of the growth only where osseous tissue with lacunæ could be found.

Imbedding in Elder Pith.—Mr. Golding Bird read a paper on the method adopted abroad of cutting sections of tissues imbedded in elder pith and packed in a microtome especially adapted for the purpose. The various steps in the operation were exhibited and explained at the same time. The principle on which the process depends is the expansion of the dried elder pith on the addition of water, so that if packed in the tube of the microtome in the dried state, and then allowed to imbibe moisture, anything previously imbedded in it is firmly gripped.

In the discussion that followed,—

Mr. Needham thought the pith would not give sufficient support on all sides.

Mr. Groves approved of the combined use of pith and wax in the way that had been shown as overcoming many difficulties in the use of wax for imbedding in a microtome, and as rendering the pith more efficient in some cases. Did not prefer a microtome that had to be held in the hand.

Mr. Giles thought the small size of the bore of the instrument might be at times objectionable. Suggested the use of dried carrot if pith could not be obtained: it would swell and soften, on the addition of water, like pith.

The Chairman objected to the pith packing in the case of diseased spinal cord, though in a healthy specimen the pressure exerted might not be deleterious. On the whole he thought the method described simple, quick, and one giving comparatively no trouble; while the microtome being held in the hand was for some reasons an advantage. He should certainly adopt the pith process in future.

Mr. Golding Bird, in reply, stated, that if properly arranged equal

support could be given to the specimen on all sides of the pith, or even the combined use of wax with pith would overcome every difficulty on that point. Thought that carrot on swelling would be too hard to cut conveniently, or would exert too much pressure. The only reason for using a microtome with small bore was to save pith; and large specimens he did not as a rule imbed, but cut by hand. Had never used pith with diseased, and therefore softened, spinal cord, but for fresh nerve tissue had seen it used with the very best results: a proper degree of hardening in some fluid first was all that was required.

Quekett Microscopical Club.

Ordinary Meeting, June 26, 1874.—Dr. Braithwaite, F.L.S., President, in the chair.

Eight members were elected.

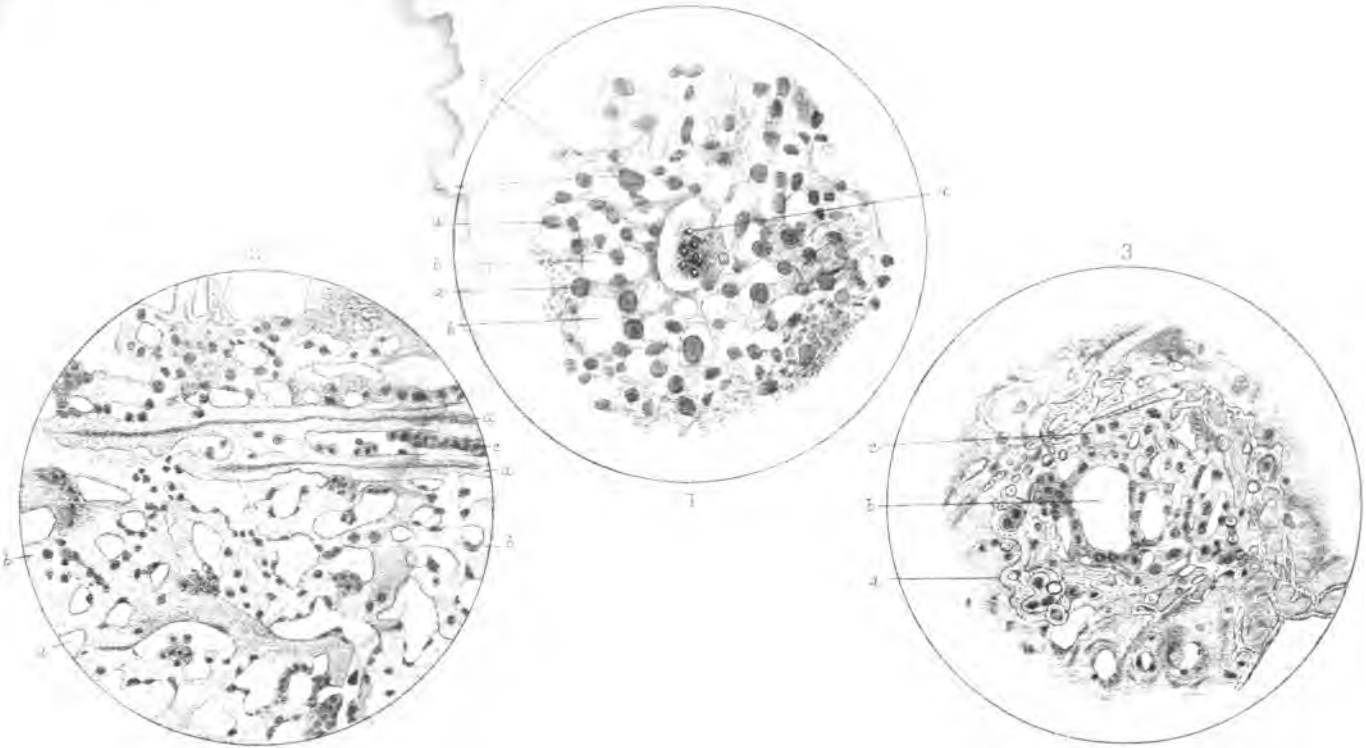
The President announced that Dr. Matthews, F.R.M.S., had been nominated by the Committee as the President for the ensuing year.

The nominations then took place of gentlemen proposed to fill the vacancies on the Committee.

A promised paper, by Mr. S. Holmes, "On Binocular Microscopes," was not forthcoming, and Mr. T. Charters White, in place of it, made some interesting remarks upon some slides which he exhibited, showing the gritty tissue of pear in a very beautiful manner.

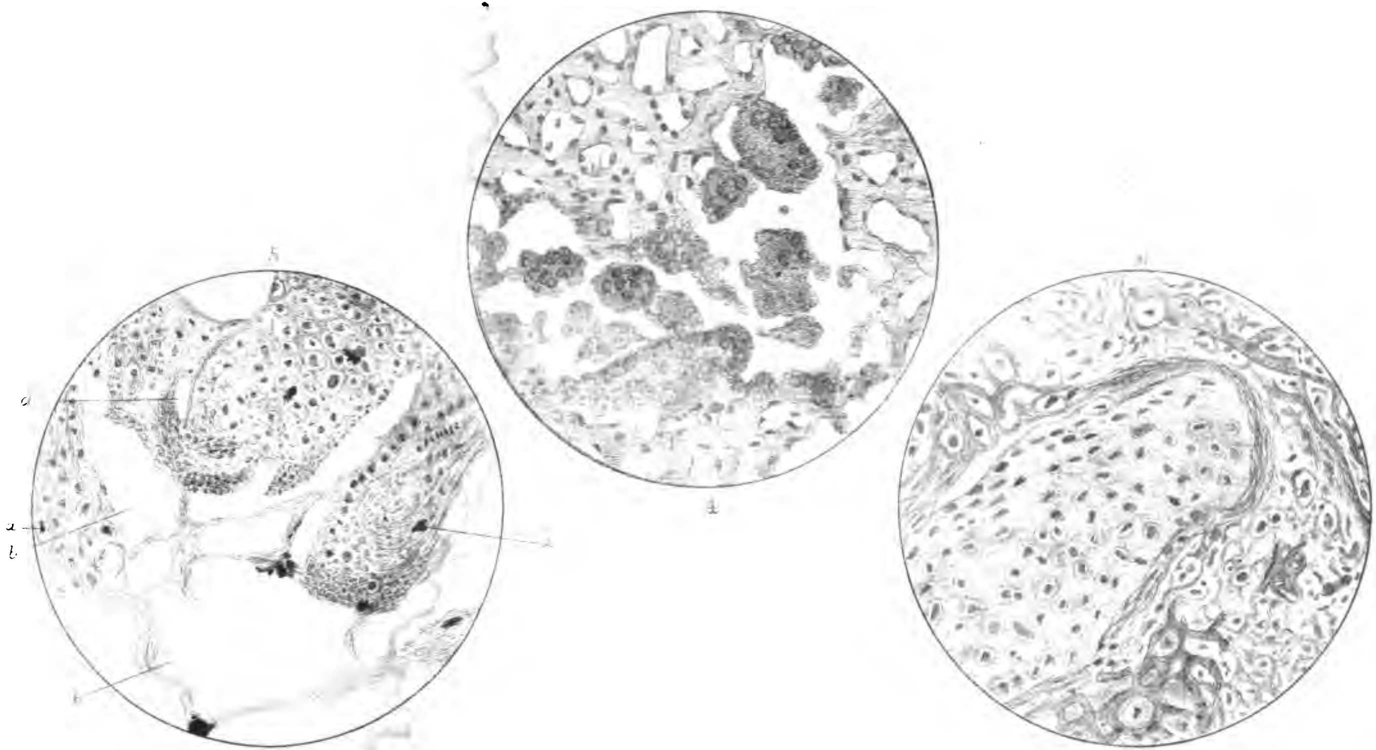
Mr. Ingpen described a form of achromatic prism, designed to replace the plane, as well as the concave mirror. In this form, the lens for condensing the light was separate, instead of being cemented to the prism, and consisted of a plano-convex achromatic doublet, the flat side of which could be placed close to one of the sides of a plane right-angled prism, or removed at pleasure, thus getting rid of the only objection to the ordinary form, viz. that it could not be used in place of the flat mirror; while its performance in other respects was unimpaired.

Various announcements of excursions, meetings, &c., were made, and several interesting objects were afterwards exhibited.



J. Needham del.t W. West & C.o lith.

Morbid growths from a case of Osteoid Cancer.



J. Needham del^t. W. West & C^o. lith.

Morbid growths from a case of Osteoid Cancer.

THE

MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1874.

I.—*On the Morbid Growths from a case of Osteoid Cancer of the Left Femur.*

By Joseph Needham, F.R.M.S., London Hospital.

(*A Paper read before the* Medical Microscopical Society, *June* 19, 1874.)

Plates LXXI. and LXXII.

The case from which these growths were obtained is described fully by Mr. W. J. Smith in the 'Medical Press,' July 8th, 1874.

The specimens presented for examination were:—

1st. Portions of the tumour surrounding the lower third of the left femur.

2nd. Portions of diseased popliteal lymphatic glands.

3rd. Part of mass representing the lumbar lymphatic glands.

4th. Several pieces of lung tissue, in which were imbedded several morbid deposits varying in size from a millet-seed to a walnut.

EXPLANATION OF PLATES LXXI. AND LXXII.

All × 200.

Fig. I.—From tumour around femur, matrix of dense uncalcified portion in transverse section—cellular elements partly brushed out.
a, Radiating fibres.
b, Irregular branching fibres.
c, Blood-vessel partly blocked up.

„ II.—Similar preparation in longitudinal section.
a, Radiating fibres.
b, Irregular branching fibres.

„ III.—From tumour around femur.
a, Zone of calcified fibres surrounding
b, a vessel whose walls are formed of
c, fibres and cells.

„ IV.—From tumour around femur.
a, A large peripheral space filled with
b, masses of cells in different stages of disintegration.

„ V.—Secondary deposit in lung.
a, Growth invading and distending
b, air-cells of lung.

„ VI.—Secondary deposit in lung.
a, Soft stroma resembling hyaline cartilage.
b, Preserved hyaline cartilage of bronchus.

1st. The consistence of this mass varies considerably; the greater part immediately surrounding the bone is very hard, whilst the external portions are soft, the density increasing from without inwards. The matrix of the dense portions consists of large irregular fibres varying in size from $\frac{1}{1000}$ to $\frac{1}{3000}$ of an inch in diameter, arranged in parallel bundles, radiating from the surface of the femur to the periphery of the tumour, but also presenting in some parts a dense irregular network, due to those radiating fibres crossing each other and uniting at various angles; each of these fibres gives off at irregular intervals branches, which radiate in various directions and join other fibres and their branches. The elements of the matrix in the central portions are so closely impacted that few interspaces can be discerned; but proceeding from this position to the circumference the fibres become more and more separated, so that they form large irregular alveoli varying in size, in which the cell elements are enclosed. In the softer parts of the tumour, more especially at the periphery, the bundles of large fibres are separated by wide intervals; the large spaces thus formed are filled by masses of the softer elements. Externally the tumour is covered by a dense layer of connective tissue, from which processes representing trabeculæ can be traced even into the dense central calcareous parts; but these processes are distinct from the fibres proper of the matrix, and in no place are they observed united. Tracing the fibres from without inwards, their structure is seen to be—first, apparently homogeneous, then faintly fibrous, and finally, calcareous; but, although this is the predominant arrangement of their structural peculiarities, yet *all* the blood-vessels are surrounded by a zone of calcified fibres. No true lacunæ nor canaliculi are to be seen, objectives up to $\frac{1}{20}$ inch revealing nothing in the so-called "osseous tissues," but minute calcareous granules and a few small badly-formed cavities. The soft elements consist of spheroidal or elongated oval cells, from $\frac{1}{3000}$ to $\frac{1}{3800}$ of an inch in diameter, each containing one or two well-defined nuclei. These cells are best seen in the large alveoli situated in the peripheral portions of the tumour; advancing to the centre, the cells become gradually smaller and more elongated, and are firmly attached to the fibres. Tracing them still further, their nuclei only are to be seen, whilst in the central calcified portions nothing approaching the character of either cell or nucleus is visible: but the cells are scattered throughout the calcareous zones surrounding the blood-vessels; closely attached to the wall, and projecting into the lumen of some of the blood-vessels, are small collections of such cells. The large peripheral spaces already described are filled with large masses of cells in different stages of disintegration; some spaces contain nothing but granular *débris*, in which minute oil-globules occupy a prominent position, while in other spaces the cells are very

granular, but little altered in shape. The tumour is not very vascular, the blood-vessels being few in number and rather large in size; their walls are very thin, and are composed of fine fibrous tissue surrounded by round and fusiform cells corresponding to those found in other parts of the growth.

2nd. Not even a trace of glandular arrangement remains to indicate the original nature of the mass; it is extremely dense, much more so than the primary growth, and consists of numerous calcareous spiculæ radiating from the centre to the circumference, forming long, narrow meshes, wherein are to be seen, closely packed, numberless round cells, agreeing in character with—but rather larger than—those of the primary deposit. In no situation do they show signs of fatty degeneration. The glands are fused together and surrounded by a capsule composed of loose connective tissue which is firmly adherent to the mass. The blood-vessels are more numerous than in the first specimen, and their walls are calcified.

3rd. In structure these glands are identical with No. 2.

4th. The structure of these masses, small and large, corresponds to that above described. The calcareous matter is deposited principally in the fibres surrounding the vessels, which are here (as in Nos. 2 and 3) more numerous than in the growth from the femur. The stroma is more loosely arranged, and in some parts it is soft, and dimly granular, resembling the matrix of hyaline cartilage; multitudes of cells are seen filling up the alveoli of the growth and distending the adjoining air-cells of the lungs; the growth thus seen invading the pulmonary alveoli consists of round nucleated cells without stroma. All that can be discerned in these morbid masses, of the normal structure of the part, is a beautifully preserved plate of hyaline cartilage, here and there the only landmark of an obliterated bronchus. The lung tissue and the morbid deposits therein are much pigmented.

The appearance of these growths (with the single exception of the absence of *true lacunæ*) agrees with those usually described under the name of peripheral osteo-sarcoma, or osteoid cancer of Müller, and as such I should regard them.

II.—*Discussion of the Formula of an Immersion Objective of greater Aperture than corresponds to the Maximum possible for Dry Objectives.* By Mr. R. Keith.

Plate LXXIII. (Lower portion).

Dr. Woodward having received from Mr. Tolles, and placed in my hands, the elements necessary for the computation of the angular aperture of the $\frac{1}{10}$-inch objective, described by him in the number of this Journal for November, 1873, p. 214, I have made the computation with five figure logarithms. A computation of this kind is much more satisfactory to mathematicians and more easily reviewed than an enlarged drawing.

The objective is composed of seven lenses. The first four, commencing at the back, are united by balsam into one combination, the next two into a middle combination, and the seventh is a hemisphere of crown glass. I give below the elements in a tabular form, and annex a figure accurately drawn to a scale. Where distances are given, the unit is in all cases an inch.

	a.	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*
Refractive index ..	1·525	1·620	1·525	1·620	1·525	1·654	1·525
Radius of 1st surface	0·265	0·200	∞	0·200	0·100	0·180	0·033
" 2nd "	0·200	∞	0·200	0·500	0·180	0·500	∞
Thickness at centre	0·048	0·027	0·033	0·047	0·062	0·020	0·035
Diameter	0·200	0·200	0·200	0·200	0·165	0·165	0·066

The distance between the first combination and the middle one is 0·008, and the radiant is assumed 10, from the first surface.

From these elements I find that the extreme ray enters the first combination 0·09250 from the axis, at an angle with the axis of 0° 31′ 45″; and enters the middle 0·07391 from the axis at an angle of − 11° 1′ 3″, the negative sign indicating convergence of the light. Adjusting the collar so that the distance of the front lens from the middle is 0·00528, I find that the same ray enters this lens 0·033 from the axis, at an angle of − 29° 44′ 7″, and makes, after refraction, an angle of − 55° 17′ 35″ with the axis.

Thus the extreme aperture, in fluid balsam, no allowance being made for the setting of the small front lens, is 110° 35′ 10″. By computation the spherical aberration for this position of the collar is practically nothing. If an allowance of only 0·00162 be made for the setting of the front lens, the aperture is reduced to 87°, and this is doubtless a proper allowance.

I append a Table giving the distances from the axis (h) at which the light crosses each surface, numbered in order, and also the

angles (E) which the ray, before crossing, makes with the axis, numbered in the same order.

Through the kindness of Dr. Woodward I am enabled to send, to the care of the Editor,* several photographic copies of the whole computation for the inspection and review of those having sufficient interest in the matter.

Surfaces.	h.	E.	Surfaces.	h.	E.
		° ′ ″			° ′ ″
1st	0·09250	+ 0 31 45	6th	0·07391	− 11 1 3
2nd	0·09139	− 6 51 51	7th	0·06625	− 24 37 5
3rd	0·08742	− 4 37 23	8th	0·05324	− 20 8 0
4th	0·08626	− 4 54 42	9th	0·03300	− 29 44 7
5th	0·08318	− 2 57 30	10th	0·00000	− 55 17 35

[* Any gentleman who is desirous of possessing one of these may do so on communicating with the Editor.—Ed. 'M. M. J.']

Georgetown, D.C., *July* 9, 1874.

III.—*Final Remarks on Immersion Apertures.*

By J. J. Woodward, Assistant-Surgeon U. S. Army.

In his reply to my article "On Immersion Objectives of greater Aperture than corresponds to *the Maximum* possible for Dry Objectives,"† Mr. Wenham asks me: "Can he show us the passage of the rays through one of the object-glasses such as he advocates in a diagram of correctly enlarged dimensions? I shall then have tangible material before me, and will enter upon the consideration with enthusiasm."‡ To this demand I replied: "If Mr. Tolles thinks proper to deviate from the ordinary practice of those who make objectives for sale, and to communicate for publication the details of the construction of either or both the objectives described in my November paper, it will be an easy matter for me to gratify Mr. Wenham, and I will endeavour to do so. Not that I think this additional testimony needed to show the accuracy of my measurements of the immersed apertures of these objectives, but because, besides the value of the information to objective makers, I should be happy to be the means of adding to the scanty store of facts which the objective makers have placed at the disposal of science." The paper in which I made these remarks § having been communicated to Mr. Tolles before its publication in England, he generously

† This Journal, November, 1873, p. 210.
‡ Ibid., December, 1873, p. 257.
§ Ibid., March, 1874, p. 121.

placed at my disposal, in a letter dated January 22nd, 1874, the details of the construction of both the objectives described in my paper in the November number of this Journal, with permission to publish so much with regard to either or both of them as might in my opinion be essential to the demonstration of the matter in dispute.

Of the two I have selected the $\frac{1}{10}$th belonging to the Museum,* both on account of its superb definition (see, for example, the photographs of *Amphipleura pellucida* sent with my November paper), and because, as this lens has a simple hemispherical front, it is an excellent example of the class of objectives with regard to which the possibility of a balsam aperture of more than 82° has been most strenuously denied. The data with regard to this objective I placed in the hands of my friend Professor R. Keith, of Georgetown, with the request that he would trace the course of the rays through the combination by the trigonometrical method, which is of course more rigidly exact than Mr. Wenham's plan of making an enlarged diagram, and tracing the rays through geometrically, and not so much more difficult as he seems to suppose.† Mr. Keith's engagements did not permit him to undertake the task at once, and after he commenced the discussion some delay was caused by lack of information with regard to one or two points, which could only be supplied by Mr. Tolles, who was absent from home on account of sickness. But for these unavoidable circumstances the results of this discussion would have been ready some time since. As it is, Mr. Keith has prepared a paper which accompanies this, and which gives the elements of the objective, and his results in detail, together with a figure reduced photographically from an enlarged diagram accurately constructed in accordance with the computed results.

It will be seen that Mr. Keith obtains in the position of the screw-collar which gives the maximum aperture, a calculated balsam angle of 110° 35′ 10″. Now, it will be remembered, that in my account of this objective I stated its balsam angle at the point of maximum, as measured by my method,‡ to be only 87°, or twenty-three degrees and a half less than the calculated angle. A moment's examination of Mr. Keith's diagram will show the reason of this, for it will be seen at once that very trifling encroachments on the diameter of the hemispherical front by its setting, will produce a considerable reduction of the angle. In fact, Mr. Keith states that he has found by actual computation an encroachment of ·00162 of an inch ($\frac{1}{600}$th very nearly) on the periphery of the front will reduce the calculated to the observed angle.

I desire also to call attention in this place to the fact, that the

* This Journal, November, 1873, p. 214.

† Ibid., April, 1873, p. 164.

‡ Ibid., June, 1873, p. 268, for the method referred to.

front discussed in my November paper* was not a complete hemisphere posteriorly, but the curve ceased at 78° from the optical axis, or 12° less than the posterior curve of the front lens of the combination discussed by Mr. Keith; as a consequence the rays proceeding backwards, after having passed through the front, will diverge less from the optical axis for any given balsam angle, in the case of the latter lens, than they do in the case of that figured in my diagram.

Mr. Keith further states that he has computed the spherical aberration of the combination, adjusted as above, and finds it practically nil. This being the case the objective ought to perform well when adjusted to the point of maximum aperture, if *balsam* be used as the immersion fluid in lieu of water, and the thick cover ordinarily employed at this position of the screw-collar. Accordingly, in company with Mr. Keith, I tested the objective in this way on *Grammatophora subtilissima* by lamplight, and we both thought the definition unmistakably better than with water immersion.

Mr. Keith has not considered it important to discuss the slight chromatic aberration which this combination is admitted to possess, because it was constructed with special reference to freedom from spherical aberration when used with monochromatic sunlight. I may say, however, that this residual chromatic aberration is not so great as to interfere in the least with the definition of the objective when used with white-cloud illumination or lamp.

I have also to record of this objective, that although it was constructed for use as an immersion lens only,† yet I have recently found by trial, that if the screw-collar be turned nearly to the open point, it performs admirably when used as a dry lens, on objects mounted dry, as well as on those mounted in balsam, provided the covers selected are of suitable thickness.‡

Let me now remind my friend Mr. Wenham of his recent promise on the subject of this controversy. "I should have been glad if Col. Woodward had given us an illustrative figure, having some relationship to a reality with the rays carried to their final destination. The passage of all his rays should have careful consideration, and if I saw no error I could not state that there is one, and trust that I have the candour to admit accuracy."§

With this I dismiss the subject; nor can I be expected to pay attention hereafter to the assertions of anyone who may continue to hold that it is "theoretically impossible" to construct immersion objectives with a balsam aperture greater than 82° "of image-forming rays," unless he can show some material error in Mr. Keith's computations. Before I conclude this paper, however, I feel called

* This Journal, November, 1873, p. 211.
† Ibid., p. 214.
‡ See Mr. Wenham's remarks, ibid., March, 1874, p. 119.
§ This Journal, April, 1874, p. 171.

upon to say a word or two with regard to certain matters which have been published in this Journal during the last few months.

And first, as to the following passage in Mr. Wenham's letter in the April number (p. 171). "The immersion focus is therefore the outer one, and the dry focus the nearest to the lens: now Col. Woodward has drawn the reverse of this, and in his diagram made the angle for immersion rays the inner one, or *closest* to the lens." To this I reply, that the focal point F* of the balsam angle 82°, corresponds *necessarily* to a dry angle only a differential less than 180°, in which the radiant must be at a point infinitely near W on the front of the objective, and that therefore in my drawing the "immersion focus is," in point of fact, "the outer one, and the dry focus the nearest to the lens," as Mr. Wenham says it ought to be, notwithstanding the circumstance that the foci of the corresponding balsam angles occupy precisely the opposite relations.

The next point to which I desire to refer briefly, is Mr. Wenham's new method of measuring apertures † by placing a vertical slit in the focus of the objective. This method might perhaps be used without giving rise to material inaccuracy when the objective is adjusted for uncovered objects; but when it is closed to the point of maximum aperture, that is in the very position about which alone there is any dispute, its spherical aberration is of course no longer corrected for uncovered objects, and if the attempt be made to focus upon them, as for instance upon the glass between the jaws of Mr. Wenham's slit, it is quite possible to make such errors in endeavouring to approximate the correct focal distance as to destroy the accuracy of the result, and lead to the unintentional cutting off of more or less of the actual angle of aperture.

Lastly, I feel compelled to refer to the paper by Mr. Brakey in the May number of this Journal (p. 221). This writer, whose peculiar style of wit owes whatever poignancy it may possess to the ready unscrupulousness with which he misrepresents the views of those whom he attacks for the purpose of trying to make them appear ridiculous, devotes four pages to a comic presentation of my opinions, and of the part I have taken in this discussion, which is characteristically inaccurate. If I could suppose that he had misunderstood my former articles, I should feel called upon to explain, but as it is evident that the misrepresentations are intentional, I shall accord to the article no further notice than this brief paragraph.

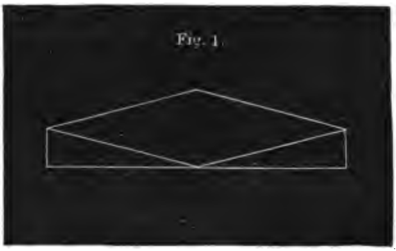
* See diagram facing p. 213, this Journal, November, 1873.

† This Journal, March, 1874, p. 112, and May, 1874, p. 198.

IV.—*Refracting Prism for Binocular Microscopes.*

By F. H. Wenham, V.P.R.M.S.

On June 13th, 1860, before the Microscopical Society, I described a Binocular Microscope with an achromatic prism of the form of Fig. 1. My communication was published in the 'Transactions' at the time. The *only* prism made on this plan was the one exhibited on that occasion.



I remarked in my paper that the drawback to this prism was "the great difficulty attendant upon its construction," for it will be seen that the upper or crown-glass prism has four facets, which have to be worked separately with all the angles exactly parallel to each other. Having made this prism myself, I had no desire to construct another of this form, and therefore in order to simplify the arrangement I transposed the components as represented by Fig. 2, in which the flint prisms are the uppermost. The angles of this diagram are drawn on the original steel templates.

Messrs. Smith and Beck fitted about a dozen microscopes with these prisms. The first was for the late Mr. Lutwidge, and the next for Mr. Janson, of Exeter. The original one, worked by myself, is now in the hands of Mr. Crisp, who has made a collection of the different forms of binoculars. Immediately afterwards the present form of *reflecting* prism was devised, which entirely superseded the last. This was described before the Microscopical Society December 12th, 1860. In that paper I state, with reference to the late refracting prism, as follows:—"Having still further advanced the definition, by a modification in the construction of the (refracting) prism, the performance was so superior to anything preceding it that several were made for parties who had seen the results, and which instruments proved satisfactory to their owners."

I find that this was the only allusion made to what I considered at the time an obsolete contrivance, and not having given a particular description, it appears to have been twice reinvented; and as in a late arrangement of erecting binocular I have only recently de-

scribed the prism,* I seem to have incurred the charge of plagiarism. The prism last employed of this form was made fourteen years ago, as I found that it could be conveniently edged down, so as to be placed deep in the setting, close behind the back lens of the object-glass.

I have no wish to disparage or criticise forms of binocular microscopes designed by others, as refracting prisms perform excellently, and perhaps the only condition that has made the now universal form so popular, is that it leaves the single body of the microscope intact, not in any way requiring a difference in the construction, or interfering with its ordinary use.

Other known constructions are all more complicated, and as the one principle appears to be the division or bisection of the object-glass by using half for each eye, and if in the main tube the direct image is straightway obtained, I do not see how definition can be improved by any intervening contrivance. The only question is whether the image in the inclined tube can be brought to the eye by any more simple means than the present reflecting prism.

V.—*On the Value of High Powers in the Diagnosis of Blood Stains.* By Joseph G. Richardson, M.D., Lecturer on Pathological Anatomy in the University of Pennsylvania, and Microscopist to the Pennsylvania Hospital.

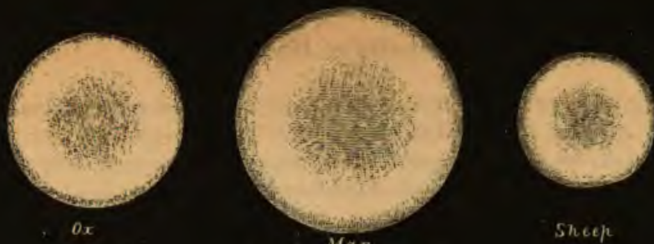
(*Read before the* Biological and Microscopical Section of the American Academy of Natural Sciences.)

Plate LXXIII. (Upper portion).

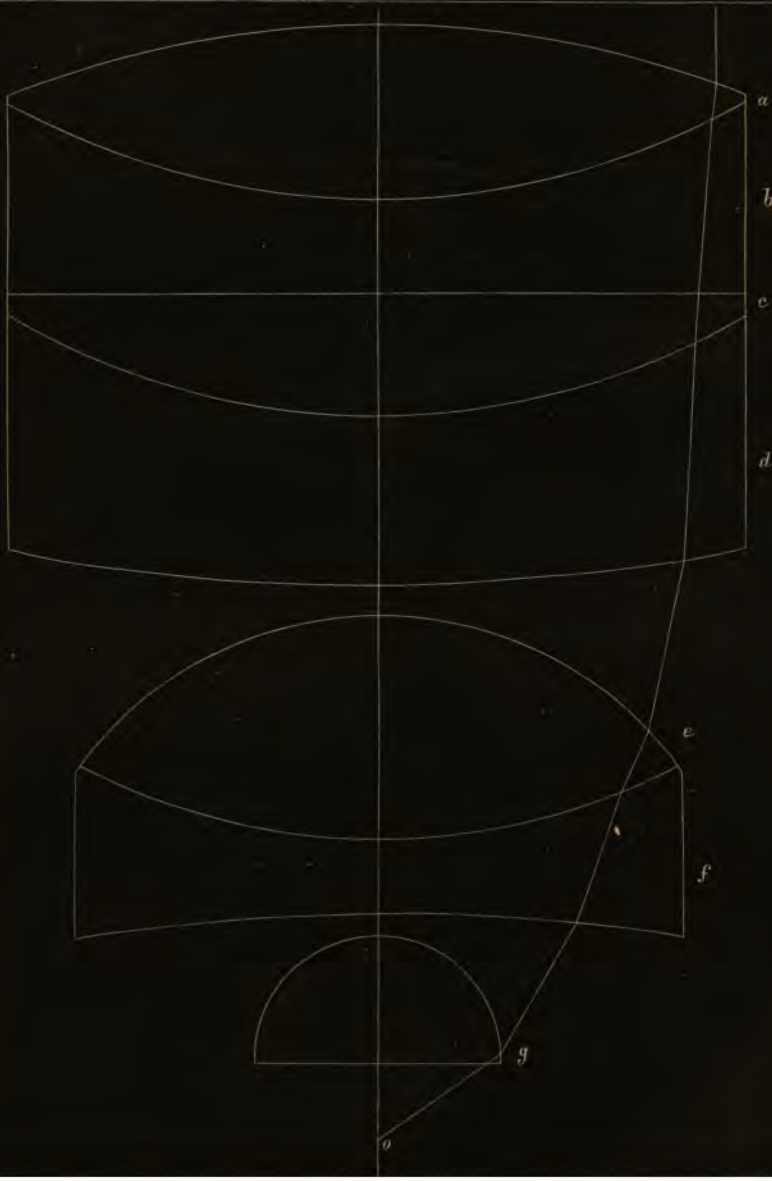
In the pages of the 'American Journal of the Medical Sciences' for July, 1869, appeared an article on the detection by the microscope, of red and white corpuscles in blood stains, in which I advocated the employment of high powers in such examination, and asserted that by their aid I had been able to demonstrate that the residuum of a dried blood-clot, left after the action of pure water, so long mistaken by Virchow, Robin, and their followers, for "pure fibrin," was composed chiefly of the *cell-walls* of the red blood-corpuscles, and that by proper management these *capsules* of the red disks could be brought clearly enough into view to enable me to measure them accurately, and so distinguish the dried blood of man from that of an ox, pig, or sheep, with a certainty disputed by Caspar, Wyman, Fleming, and other previous observers.

This possibility of recognizing blood-globules when dried *en masse*, is of course closely associated with, if not actually dependent upon, their possession of a cell-wall, as maintained in my paper on the cellular structure of the red blood-corpuscle, in the 'Trans. of the Am. Med. Assoc.' for 1870 (the theory being mainly deduced

* 'M. M. J.,' May, 1873, p. 218.



× 3700 Diameters
(1/50th B-Eyepiece)



from experiments upon the gigantic blood-disks of the Menobranchus, in which crystals of hæmato-crystallin were seen to prop out a visible membranous capsule). Indeed, as I have elsewhere remarked, if the red blood-globules are simply homogeneous lumps of jelly-like matter, the chance of discovering any individual corpuscles in a mass of dry blood-clot, however moistened, seems almost as hopeless as the search after individual rain drops in a cake of melting ice.

Notwithstanding this, however, we find in the third edition of Prof. A. S. Taylor's work on Medical Jurisprudence,* figures of red blood-corpuscles of ten different animals, as they appear under a low power, with the statement (strictly accurate in regard to blood-disks thus *feebly* magnified) that "there are no certain methods of distinguishing, microscopically or chemically, the blood of a human being from that of an animal, when it has been once dried on an article of clothing." This declaration seems to show that more complete and conclusive proof is still needed of the superior advantage derivable from the application of high objectives to the diagnosis of blood stains.

The *a priori* arguments against the value of this microscopic test for distinguishing human blood from that of the ox, pig, horse, sheep, and goat, may be grouped under three heads, viz. :—1st. It is objected, as by Taylor, Caspar, and others, that the difference between the red blood-corpuscles of man and of these domestic animals, is too minute to render their positive discrimination possible, and too insignificant to admit of its being used as the means of condemning a fellow-creature to death. 2nd. That even if the average diameters of these various corpuscles were shown to be so different that we might sometimes by this means distinguish them, yet the variations above and below the mean diameter are so frequent and irregular, that they must render the determination of any such averages by mere micrometric measurement unreliable; and 3rd, many investigators believe, with Virchow and Brücke, that no microscopist can "hold himself justified in putting in question a man's life on the uncertain calculation of a blood-corpuscle's ratio of contraction by drying."

In reply to the first of these objections, it may be urged that the blood-corpuscles are just as much characteristics of the different kinds of living beings in which they occur, as are the coverings of the body, the shape of the legs, or the number of joints in the antennæ, so that exactly as we may tell, for example, a bird's skin from an animal's, by the former being covered with feathers, whilst the latter is furnished with hair, so we may distinguish a bird's or a camel's blood from that of a man, by the former having oval corpuscles, whilst those of the latter are rounded in their outline.

* Vol. i., p. 548.

Further, in regard to the red blood-disks of animals with rounded corpuscles, I can perhaps best illustrate the principles that guide us in their discrimination by suggesting that these bodies may be aptly compared to different sizes of shot. Thus, for instance, the red globules of man's blood are nearly twice the size of the sheep's, and about four times that of the musk deer's, just as No. 1 shot is perhaps double the magnitude of No. 5 and quadruple that of No. 8.

It is obvious, too, that a shot dealer in the latter case, or a skilful microscopist in the former, would more quickly and surely *distinguish* two analogous sizes of red blood, or of leaden *globules*, from each other, than could an inexperienced apprentice in either occupation.

Hence it follows, that whilst we might be in doubt whether the shot dissected out of the body of a wounded man was a No. 1 or a No. 2, we could have no hesitation, after measuring it with a gauge, in declaring it was too large for a No. 5 and *a fortiori* for a No. 8, precisely as the corpuscles of man's blood might be confounded with those of a monkey's, but on measurement are seen at once to be too large for those of an ox or sheep. Nor can it be disputed that *mere measurement* in either instance, when practically correct, is quite sufficient to decide a doubtful case, as, for example, if I was to shoot myself in the hand, and then assert that it had been done by some one else, whose gun was known to be loaded with No. 8 shot, whilst the grains in my flesh were actually of the size of No. 1.

It must be remembered, too, that whilst the relative differences between corpuscles of human, ox, and sheep's blood remain the same, the absolute difference becomes more perceptible in proportion as the disks are magnified above, for example, those represented in Dr. Taylor's work, so that when the former corpuscles appear $\frac{8}{8}$ of an inch and the latter $\frac{5}{8}$ of an inch across (as they do under the $\frac{1}{50}$), they can hardly be mistaken for another, any more than a 12-inch shell could be mistaken for a 6-inch shell, even by a careless person, who would call a No. 1 a No. 5 shot.

Ordinarily in criminal cases the microscopist is called upon to determine, not whether a particular specimen is human, as distinguished from all other kinds of blood, but to discriminate simply between the blood-corpuscles of a man and an ox, a man and a horse, or a man and a sheep, and so establish or disprove the defendant's story as to how his clothing or other articles became stained with blood. Sometimes the much easier task is imposed (as in a recent case wherein I was engaged) of diagnosing between the blood of a human being and that of a bird.* In this instance

* Trial of Charles Larribee for the murder of Lewis Williams, at Franklin, Venango Co., Pa., see 'Oil City Daily Derrick,' May 1st, 1874.

many of the suspected stains occurred on the prisoner's boots, and proved upon that article of clothing singularly easy of detection.

Finally, I would remind those who demur at the idea of allowing a man's life to hang upon such seemingly insignificant circumstances as a difference in size of blood-corpuscles, how often the reactions of arsenic, afforded by a quantity of the metal too excessively trivial to be accurately estimated by the most delicate balance, have sufficed to bring out the crime of murder, and to aid in securing that just punishment for violation of law in which we all have so deep an interest, because on it all our enjoyment of life and property depends.

To the second objection, viz. that the variations above and below the standard size of corpuscles from any particular animal are too great and irregular to permit us to obtain an accurate result by measurement, I would answer, that this difference in size is more especially observable in corpuscles dried in a thin film upon a glass slide, and is then probably in part a pathological change due to external violence in spreading and drying. These variations are comparatively slight in fresh blood, as is proved by the following experiments, made with my $\frac{1}{50}$th inch objective, which gives with the micrometer eye-piece an amplification of 3700 diameters. When thus magnified the human red blood-disks appear about one inch and one-eighth in diameter, so that even slight differences in their size can be accurately measured. Among one hundred red corpuscles freshly drawn from five different persons, the maximum, minimum, and mean diameters were as follows:—

	Max.	Min.	Means.
Twenty from a white male aged 30	1-3231	1-3500	1-3355
" " " " " 38	1-3281	1-3529	1-3375
" " " female " 44	1-3249	1-3500	1-3381
" " an African " " 50	1-3182	1-3559	1-3384
" " a white male " 8	1-3231	1-3500	1-3398
Average of means			1-3378

The measurement of twenty corpuscles from part of the first of these specimens dried in a thin film upon a slide gave a maximum of $\frac{1}{2800}$, a minimum of $\frac{1}{3621}$, and a mean diameter of $\frac{1}{3182}$ of an inch.

Moreover, if it can be shown that the smallest red disks of man, as usually met with in mechanically unaltered blood, whether dry or moist, are larger than the largest corpuscles of an ox, and *a fortiori* of a sheep, such an objection, as regards these particular animals at least, becomes valueless, and that this is the case I propose to presently demonstrate.

As illustrating the accuracy which some practical experience in measuring minute objects, like the red blood-disks, with the cobweb micrometer enables us to attain, I may instance the following fact,

which my friend, Prof. Theodore G. Wormley, M.D., of Columbus, Ohio, kindly permits me to mention here, but which may appear more in detail in his appendix on blood stains to the next edition of his splendid work on 'Micro-Chemistry of Poisons.' During a recent visit to Philadelphia Prof. Wormley brought with him a slide of human blood, upon which were seven corpuscles (designated by numbers on an accompanying drawing), which he had measured under several different objectives and forms of apparatus. These corpuscles Dr. W. requested me to measure under my $\frac{1}{25}$ immersion lens, and after doing so I found that my results agreed very closely with his own, and that in two or three instances they were précisely identical. The mean diameter of the seven disks, according to my computation, was $\frac{1}{3268}$ against $\frac{1}{3236}$ of an inch, the average of his measurements. There was thus a total deviation from the true size of only $\frac{1}{352292}$ of an inch in my results, which were those of an independent observer, seeing the objects for the first time, and determining their magnitude under a magnifying power, and by the aid of apparatus entirely different from those Prof. Wormley had employed.

Thirdly, the assertion of Virchow, that a man's life should not be put in question on the uncertain calculation of a blood-corpuscle's ratio of contraction by drying, does not seem to me a fair statement of the point at issue; because since the red blood-corpuscles of oxen, horses, pigs, sheep, deer, and goats are all much smaller than those of man, no degree of *contraction* which they could undergo would render the stains in which they occur *more* liable to be mistaken for man's blood; and if, as is rarely, if ever, the case, human red blood-corpuscles in a stain were by any means contracted so as to resemble those of an ox, for instance, in size, the evidence from microscopic examination would only mislead us into assisting in the acquittal of a criminal, and could not betray us into aiding to convict an innocent person.

Had Prof. Virchow worded his statement so as to read, "the uncertain calculation of a blood-corpuscle's ratio of contraction *or expansion* by drying," his objection would have been strictly logical, although, as I believe, it would not have been founded upon fact, because if a corpuscle of ox blood could *expand* during the process of desiccation or of moistening so as to even approximate to the human red disk in magnitude, it might mislead us into testifying erroneously to the presence of man's blood, when beef blood alone had been shed, and thereby endangering the life of an individual who was entirely guiltless.

But my observations, made upon many different kinds of blood, and under a great variety of conditions, clearly indicate that the cell-wall of a red blood-globule is nearly or quite inelastic, and incapable of any marked expansion by the process of drying or moistening

with the fluids I recommend for the examination of blood stains. The slight increase of size previously mentioned as occurring in the desiccation of a thin film of blood, forms, I believe, only an apparent exception, and is probably due to a change of shape taking place during the complete flattening out of the disks as they lose their contained water. The experience of Prof. Leidy and Prof. Wormley accords with mine, in that they have never seen the drying or remoistening of red blood-corpuscles cause them to expand, and I therefore conclude we may affirm that when the corpuscles remain uncontracted, their indications are perfectly reliable, and if they shrink (as I believe they rarely do), that being the only serious modification which they can undergo, the sole danger is that by a possible, but not probable, mistake in diagnosis of the origin of a blood stain through contraction of its corpuscles, we might contribute to a criminal's escape, *never* to the punishment of an innocent party.

But all these theoretical considerations are of very secondary importance in comparison with the positive fact, as to whether practically we can or cannot discriminate the stains of human blood from those made by the blood of oxen and sheep. I have therefore endeavoured to *work out* a conclusive answer to this question, obtaining it by a method which will, I trust, carry conviction to the mind of every honest seeker after truth.

On the 16th of May, 1874, my friends, Prof. J. J. Reese and Dr. S. Weir Mitchell, each kindly prepared for me three packages of dried blood from stains made by sprinkling the fresh fluid from an ox, a man, and a sheep, upon white paper. The two series were simply numbered 1, 2, and 3, and a memorandum preserved by each gentleman, specifying which kind of blood composed each sample. By this plan it is obvious that I was prevented from having any clue to the origin of the specimens save that afforded by the microscope, and my examinations and measurements were therefore entirely free from bias.

Some small particles from specimen No. 1, handed me by Prof. Reese, were broken up into a fine dust, with a sharp knife upon a slide, and covered with a film of thin glass. A few drops of the ordinary three-quarter of 1 per cent. common salt solution were then successively introduced at one margin of the cover, and removed from the opposite edge, as they penetrated thither, by a little slip of blotting-paper, thus washing away the colouring matter from the tiny masses of dried clot. When these particles were nearly decolourized, a drop of aniline solution was allowed to flow in beneath the cover, and, after remaining about half a minute, was in its turn washed away, and its place supplied by a further portion of weak salt solution.

On adjusting the specimen as thus prepared, under a $\frac{1}{25}$ immer-

sion lens (giving an amplification, with the A eye-piece, of 1250 diameters), a fragment of the blood stain was soon discovered, which displayed the delicate cell-walls of its component red and white corpuscles, as figured in my 'Handbook of Medical Microscopy,' p. 284. Ten consecutive red disks from these, selected simply as among those which had become but little distorted, were found to measure as noted below in the first column. The second and third rows of figures show the result of similar experiments, performed on samples 2 and 3, all the magnitudes being given in parts of an English inch.

Specimen No. 1.	Specimen No. 2.	Specimen No. 3.
1-3448	1-4762	1-5555
1-3572 (minimum)	1-4762	1-6060
1-3572	1-4878 (minimum)	1-5405 (maximum)
1-3572	1-4651	1-5880
1-3333	1-4878	1-6666 (minimum)
1-3125 (maximum)	1-4444 (maximum)	1-6060
1-3448	1-4444	1-5777
1-3278	1-4762	1-5555
1-3333	1-4651	1-5888
1-3448	1-4762	1-5777
1-3407 (mean)	1-4694 (mean)	1-5828 (mean)

Since the red corpuscles of human, ox, and sheep's blood measure, according to Gulliver, $\frac{1}{3200}$, $\frac{1}{4267}$, and $\frac{1}{5300}$ of an inch respectively, and previous experiments of my own had demonstrated a disposition to *slight* contraction in the corpuscles of blood stains which have been dried and moistened again, I of course concluded that sample No. 1 was human blood, No. 2 was ox blood, and No. 3 was sheep's blood. On reporting these diagnoses to Prof. Reese, I had the satisfaction of learning that they were "entirely correct."

Careful examination of the three specimens furnished me by Dr. Mitchell, and prepared in a manner similar to that detailed above (except that diluted liq. iodinii comp. was used instead of aniline liquid for tinting the cellular elements), led me to analogous conclusions, as will be seen from the following table of measurements:—

Specimen No. 1.	Specimen No. 2.	Specimen No. 3.
1-4545	1-6250	1-3572
1-4762	1-6250	1-3390
1-4878 (minimum)	1-6060	1-3175 (maximum)
1-4347 (maximum)	1-6450 (minimum)	1-3278
1-4444	1-5880	1-3448
1-4762	1-5777	1-3333
1-4651	1-5555	1-3572
1-4878	1-5405 (maximum)	1-3390
1-4545	1-6250	1-3572 (minimum)
1-4878	1-5880	1-3636
1-4662 (mean)	1-5952 (mean)	1-3430 (mean)

From these results, I of course decided that No. 1 was ox blood, No. 2 was sheep's blood, and No. 3 was human blood, and on reporting my conclusions to Dr. Mitchell, I was again very much gratified to receive a reply informing me that they were perfectly correct.

It is interesting and important to observe, that in no instance do the minimum diameters of the human blood-corpuscles closely approach the maximum diameter of even those from ox blood. It is true that corpuscles are occasionally to be met with both in fresh blood and in dry clot, which fall much below the general average of the specimen, but these are comparatively rare (not amounting to over one in a hundred), and they so generally in fresh blood bear such marks of traumatic injury or pathological change, that it is only fair to disregard them in making up our estimates. If my views are correct respecting the osmotic processes constantly going on through the cell-wall of both the red and the white corpuscles,* alterations in the specific gravity of the liquor sanguinis, surrounding the corpuscles, produced by desiccation at the margin of the thin glass cover, must cause slight changes in the diameter of the disks. Nevertheless, as these variations necessarily lie between their normal size ($\frac{1}{3378}$?), and their magnitude when dried upon a slide ($\frac{1}{3182}$?), they can never lead to confusion in diagnosis even from ox blood.

In regard to the practical minutiæ of the examination of blood stains, I have little to add to the description given a page or two back, except concerning the menstrua advised by various authors.

The saturated solution of sulphate of soda recommended by Prof. Charles Robin, and endorsed by numerous authorities, has the disadvantage of rapidly crystallizing around the specimen, and must, I think, owe its popularity chiefly to the fact that it often contains large quantities of a peculiar fungus, the spores of which closely resemble red blood-corpuscles both in size and general appearance, and have, I doubt not, frequently been mistaken for blood-cells. Diluted albumen and solution of hypophosphite of soda have not in my hands seemed to possess any peculiar advantages, and the method of Erpenbeck, quoted by Prof. Taylor, of gently breathing on the fragments of blood-clot until they are sufficiently moistened to liquefy, will not, I believe, in general enable us to demonstrate any corpuscles except the leucocytes of the coagulum. These leucocytes have probably often been mistaken by observers for "decolourized red disks."

The highly refractive properties of glycerin and its solutions advised by Dr. Taylor and others, render it in my judgment less

* Vide 'Report on the Structure of the White Blood-corpuscle,' "Trans. of Am. Med. Assoc.," 1872, p. 178.

applicable as a liquid for moistening blood stains and bringing into view the delicate cell-walls of their constituent corpuscles than the ·75 per cent. salt solution. I can, however, fully agree with my friend, Dr. R. M. Bertolet, that for preservation and prolonged study of specimens of blood stains, glycerin forms the best medium at our disposal, although it seems to me that his suggestion, that we should before mounting them tint the cell-walls and nuclei of oviparous blood-corpuscles with the reagents employed in the admirable guaiacum test for blood, will be found in practice less advantageous than my own plan of using aniline solution. And this in part on account of the difficulty of procuring the ethereal proportion of peroxide of hydrogen, and of applying it to microscopic specimens, and partly because it will prove so much harder to convince the average juryman that a bright blue material (instead of a crimson-red substance) is actually clotted blood.

In examining spots of blood more than one-tenth of an inch in diameter, I would advise that fragments should be scraped from the edges or thinnest parts of the stain, because specimens from the central portions sometimes exhibit numerous fibrin filaments which have appeared before the desiccation of the drop. These of course interfere with the investigation by forming a more or less complete meshwork around the cell-walls, and so confusing the delicate outlines which the latter present when the view is uninterrupted.

As a contribution towards answering the question of how long after their deposit upon objects blood stains may be detected by microscopic investigation, I may mention that a fragment from one of the twenty blood spots used in May 1869 "for estimating the delicacy of the microscopic test for blood" (determined at $\frac{1}{12000}$ of a grain, as stated in my paper in vol. lviii., N. S., of the 'American Journal of the Medical Sciences,' p. 57) was recently examined as above described, and found still at the end of *five years* to exhibit multitudes of corpuscles, which could be clearly distinguished from those of the ox or sheep, as will be seen by the following record of measurements made May 23, 1874:—

1-3572 of an inch.		
1-3448	„	
1-3278	„	
1-3125	„	maximum.
1-3390	„	
1-3509	„	
1-3448	„	
1-3509	„	
1-3572	„	minimum.
1-3448	„	
1-3425	„	mean of ten corpuscles.

The corresponding average of my measurements five years ago was $\frac{1}{3474}$ of an inch, so that no further contraction seems to result

from age, and as the outlines of the corpuscles appear quite as distinct now as they did soon after the blood was drawn, it seems probable that this microscopic evidence of human bloodshed will be equally unmistakable twenty or even fifty years hence, provided due care continues to be exercised in its preservation from moisture and external violence.

In conclusion, I submit, that the results of my experiments above narrated *prove that*, since the red blood-globules of the pig ($\frac{1}{4230}$), the ox ($\frac{1}{4267}$), the red deer ($\frac{1}{4324}$), the cat ($\frac{1}{4404}$), the horse ($\frac{1}{4600}$), the sheep ($\frac{1}{5300}$), and the goat ($\frac{1}{6366}$ of an inch), are all so much smaller than even the ordinary minimum size of the human red disk, as measured in my investigations, *we are now able by the aid of high powers of the microscope*, and under favourable circumstances, to positively distinguish stains produced by human blood from those caused by the blood of any of the animals just enumerated, and this even after the lapse of five years from the date of their primary production.

No. 1620, Chestnut Street, Philadelphia.

VI.—*An Account of certain Organisms occurring in the Liquor Sanguinis.* By William Osler, M.D.

Plate LXXIV.

In many diseased conditions of the body, occasionally also in perfectly healthy individuals and in many of the lower animals, careful investigation of the blood proves that, in addition to the usual elements, there exist pale granular masses, which on closer inspec-

---

EXPLANATION OF PLATE LXXIV.

Fig. 1.—Common forms of the masses from healthy blood. (Ocular 3, Objective 5.)

„ 2.—A mass from healthy blood, in saline solution, showing stages of development; *a*, at 10 A.M.; *b*, at 10.30 A.M.; *c*, at 11 A.M. (Ocular 3, Objective 7.)

„ 3.—Mass from blood of young rat (in serum) in full development, after two hours' warming. (Ocular 3, Objective 7.)

„ 4.—Mass (young rat) with blood-corpuscles about it, to show the relative sizes. (Ocular 3, Objective 5.)

„ 5.—Some of the developed forms as seen with No. 11 Hartnack. (See text.)

„ 6.—Form watched for four hours. (Ocular 3, Objective 9.)

„ 7.—Form watched for five hours. (Ocular 3, Objective 9.)

„ 8.—Small vein in connective tissue from the back of a young rat, showing the corpuscles free among the red ones. (Ocular 3, Objective 7.)

„ 9.—Small vein from the connective tissue of a rat (in serum), showing corpuscles and developed forms. (Ocular 3, Objective 9.)

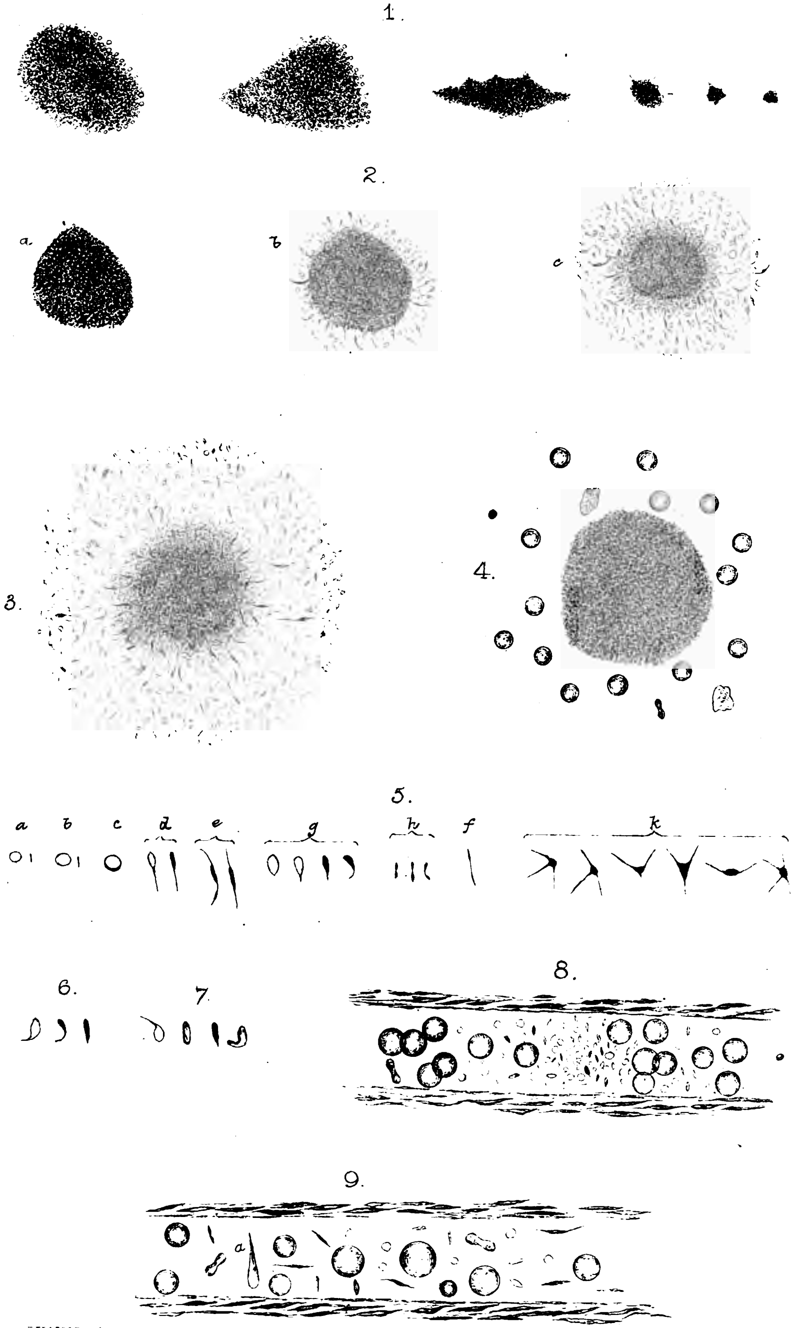
tion present a corpuscular appearance (Pl. LXXIV., Fig. 1). There are probably few observers in the habit of examining blood who have not, at some time or other, met with these structures, and have been puzzled for an explanation of their presence and nature.

In size they vary greatly, from half or quarter that of a white blood-corpuscle, to enormous masses occupying a large area of the field, or even stretching completely across it. They usually assume a somewhat round or oval form, but may be elongated and narrow, or, from the existence of numerous projections, offer a very irregular outline. They have a compact solid look, and by focussing are seen to possess considerable depth; while in specimens examined without any reagents the filaments of fibrin adhere to them, and, entangled in their interior, white corpuscles are not unfrequently met with.

It is not from every mass that a judgment can be formed of their true nature, as the larger, more closely arranged ones have rather the appearance of a granular body, and it is with difficulty that the individual elements can be focussed. When, however, the more loosely composed ones are chosen, their intimate composition can be studied to advantage, especially at the borders, where only a single layer of corpuscles may exist; and when examined with a high power (9 or 10 Hartnack) these corpuscles are seen to be pale round disks, devoid of granules and with well-defined contours. Some of the corpuscles generally float free in the fluid about the mass; and if they turn half over, their profile view has the appearance of a sharp dark line (Fig. 5, *a* and *b*). In water the individual corpuscles composing the mass swell greatly; dilute acetic acid renders them more distinct, while dilute potash solutions quickly dissolve them. Measurements give, for the large proportion of the corpuscles, a diameter ranging from $\frac{1}{8000}$th to $\frac{1}{10000}$th of an inch; the largest are as much as $\frac{1}{5000}$th, and the smallest from $\frac{1}{15000}$th to $\frac{1}{24000}$th of an inch; so that they may be said to be from $\frac{1}{8}$th to $\frac{1}{2}$ the size of a red corpuscle. In the blood of cats, rabbits, dogs, guinea-pigs, and rats, the masses are to be found in variable numbers. New-born rats are specially to be recommended as objects of study, as in their blood the masses are commonly both numerous and large. They occur also in the blood of fœtal kittens.

Considering their prevalence in disease and among some of the lower animals, they have attracted but little notice, and possess a comparatively scanty literature. The late Professor Max Schultze * was the first, as far as I can ascertain, to describe and figure the masses in question. He speaks of them as constant constituents of the blood of healthy individuals, but concludes that we know nothing of their origin or destiny, suggesting, however, at the same

* Archiv f. mik. Anat., Bd. i.



W. H. Wesley Sc.

W. West & Co. imp.

Organisms in the liquor Sanguinis.

time that they may arise from the degeneration of granular white corpuscles. Schultze's observations were confined to the blood of healthy persons, and he seemed of the opinion that no pathological significance was to be attributed to them.

By far the most systematic account is given by Dr. Riess,* in an article in which he records the results of a long series of observations on their presence in various acute and chronic diseases. His investigations of the blood of patients, which were much more extensive than any I have been able to undertake, show that, in all exanthems and chronic affections of whatever sort, indeed, in almost all cases attended with disturbance of function and debility, these masses are to be found. He concludes that their number is in no proportion to the severity of the disease, and that they are more numerous in the latter stages of an affection, after the acute symptoms have subsided. The former of these propositions is undoubtedly true, as I have rarely found masses larger or more abundant than I, at one time, obtained from my own blood when in a condition of perfect health. These two accounts may be said to comprise everything of any importance that has been written concerning these bodies. The following observers refer to them cursorily:—Erb,† in a paper on the development of the red corpuscles, speaks of their presence under both healthy and diseased conditions: he had hoped, in the beginning of his research, that they might stand, as Zimmerman supposes (see below), in some connection with the origin and development of the red corpuscles; but, as he proceeded, the fallacy of this view became evident to him. Bettelheim ‡ seems to refer to these corpuscles when he speaks of finding in the blood of persons, healthy as well as diseased, small punctiform, or rod-shaped, corpuscles of various sizes. Christol and Kiener § describe in blood small round corpuscles, whose measurements agree with the ones under consideration; and they also speak of their exhibiting slight movements. Riess,‖ in a criticism on a work of the next-mentioned author, again refers to these masses, and reiterates his statements concerning them. Birsch-Hirschfeld ¶ had noticed them and the similarity the corpuscles bore to micrococci, and suggests that under some conditions *Bacteria* might develop from them. Zimmerman ** has described corpuscular elements in the blood, which, with reference to the bodies in question, demand a notice here. He let blood flow directly into a solution of a neutral salt, and, after the subsidence

* Reichert u. Du Bois-Reymond's Archiv, 1872.
† Virchow's 'Archiv,' Bd. xxxiv.
‡ 'Wiener med. Presse,' 1868, No. 13.
§ 'Comptes Rendus,' lxvii., 1054. Quoted in 'Centralblatt,' 1869, p. 96.
‖ 'Centralblatt,' 1873, No. 34.
¶ 'Ibid., 1873, No. 39.
** Virchow's 'Archiv,' Bd. xviii.

of the coloured elements, examined the supernatant serum, in which he found, in extraordinary numbers, small, round, colourless corpuscles with weak contours, to which he gave the name of "elementary corpuscles." These he met with in human blood both in health and disease, and in the blood of the lower animals; and he found gradations between the smaller (always colourless) forms and full-sized red corpuscles. He gives measurements (for the smaller ones, from $\frac{1}{1000}$th to $\frac{1}{800}$th of a line; the largest, $\frac{1}{500}$th to $\frac{1}{400}$th of a line), and speaks of them also as occurring in clumps and groups of globules. It is clear, on reading his account, that in part, at any rate, he refers to the corpuscles above described. Gradations such as he noticed between these and the coloured elements I have never met with, and undoubtedly he was dealing with the latter in a partially decolourized condition. Lostorfer's * corpuscles, which attracted such attention a few years ago, from the assertion of the discoverer that they were peculiar to the blood of syphilitic patients, require for their production an artificial culture in the moist chamber, extending over several days. They appear first after two or three days, or even sooner, as small bright corpuscles, partly at rest, partly in motion, which continue to increase in size, till by the sixth or seventh day they have obtained the diameter of a red corpuscle, and may possess numerous processes or contain vacuoles in their interior. Blood from healthy individuals, as well as from diseases other than syphilis, has been shown to yield these corpuscles; and the general opinion at present held of them is that they are of an albuminoid nature.

The question at once most naturally arose, How is it possible for such masses, some measuring even $\frac{1}{400}$th of an inch, to pass through the capillaries, unless supposed to possess a degree of extensibility and elasticity, such as their composition hardly warranted attributing to them? Neither Max Schultze nor Riess offer any suggestion on this point, though the latter thinks that they might, under some conditions, produce embolism.

During the examination of a portion of loose connective tissue from the back of a young rat, in a large vein which happened to be in the specimen, these same corpuscles were seen, not, however, aggregated together, but isolated and single among the blood-corpuscles (Fig. 8); and repeated observations demonstrated the fact that in a drop of blood taken from one of these young animals, the corpuscles were always to be found accumulated together; while on the other hand, in the vessels (whether veins, arteries, or capillaries) of the same rat they were always present as separate elements showing no tendency to adhere to one another. The masses, then,

* 'Wiener med. Presse,' 1872, p. 93. 'Wiener med. Wochenschrift,' 1872, No. 8. Article in Archiv f. Dermatolog., 1872.

are formed at the moment of the withdrawal of the blood, from corpuscles previously circulating free in it.

To proceed now to the main subject of my communication. If a drop of blood containing these masses is mixed on a slide with an equal quantity of saline solution, $\frac{1}{2}$ to $\frac{3}{4}$ per cent., or, better still, perfectly fresh serum, covered, surrounded with oil, and kept at a temperature of about 37° C., a remarkable change begins in the masses. If one of the latter is chosen for observation, and its outline carefully noted, it is seen, at first, that the edge presents a tolerably uniform appearance, a few filaments of fibrin perhaps adhering to it, or a few small corpuscles lying free in the vicinity. These latter still exhibit apparent Brownian movements, frequently turning half over, and showing their dark rod-like border (Fig. 5, *a*, *b*). After a short time an alteration is noticed in the presence of fine projections from the margins of the mass, which may be either perfectly straight, or each may present an oval swelling at the free or attached end or else in the middle (Fig. 2, *b*). It is further seen that the edges of the mass are now less dense, more loosely arranged, or, if small, it may have a radiant aspect. Sometimes, before any filaments are seen, a loosening takes place in the periphery of the mass, and among these semi-free corpuscles the first development occurs. The projecting filaments above-mentioned soon begin a wavy motion, and finally break off from the mass, moving away free in the fluid. This process, at first limited, soon becomes more general; the number of filaments which project from the mass increases, and they may be seen not only at the lateral borders, but also, by altering the focus, on the surface of the mass, as dark, sharply-defined objects. The detachment of the filaments proceeds rapidly, and in a short time the whole area for some distance from the margins is alive with moving forms (Fig. 2, *c*, and Fig. 3), which spread themselves more and more peripherally as the development continues in the centre. In addition to the various filaments, swarming granules are present in abundance, and give to the circumference a cloudy aspect, making it difficult to define the individual forms. The mass has now become perceptibly smaller, more granular, its borders indistinct and merged in the swarming cloud about them; but corpuscles are still to be seen in it, as well as free in the field. A variable time is taken to arrive at this stage; usually, however, it takes place within an hour and a half, or even much less. The variety of the forms increases as the development goes on; and whereas, at first, spermatozoon-like or spindle-shaped corpuscles were almost exclusively to be seen, later more irregular forms appear, possessing two, three, or even more, tail-like processes of extreme delicacy (Fig. 5, *k*). The more active ones wander towards the periphery, pass out of the field,

and become lost among the blood-corpuscles. The process reaches its height within 2½ hours, and from this time begins almost imperceptibly to decline; the area about the mass is less densely occupied by the moving forms, and by degrees becomes clearer, till at last, after six or seven hours (often less), scarcely an element is to be seen in the field, and a granular body, in which a few corpuscles yet exist, is all that remains of the mass. The above represents a typical development from a large mass in serum, such as that seen in Fig. 3.*

We have next to study more in detail the process of development and the resulting forms. Commonly, the first appearance of activity is displayed by the small free corpuscles at the margins, which, previously quiescent, begin a species of jerky irregular movement, at one time with their pale disk-surfaces uppermost, at another presenting their dark linear profiles (Fig. 5, *a* and *b*). Not unfrequently some of these are seen with a larger or smaller segment of their circumference thicker and darker than the other (Fig. 5, *c*).

Earliest, and perhaps the most plentiful, of the forms are those of a spermatozoon-like shape (Fig. 5, *d*), attached to the mass either by the head or tail; while, simultaneously, long bow-shaped filaments appear (Fig. 5, *e*), having an enlargement in the centre. Straight hair-like filaments (Fig. 5, *f*) may also be seen, but they are not very numerous. The time which elapses before they begin the wavy movement is very variable, as is also the time when they break away after once beginning it. Filaments may be seen perfectly quiescent for more than half an hour before they move, and others may be observed quite as long in motion before they succeed in breaking away from the mass. Commonly it is in the smaller masses, and where the development is feeble, that filaments remain for any time adherent. The spermatozoon-like forms appear, at the head, on one view flattened and pale, on the other dark and linear (Fig. 5, *d*); consequently the head is discoid, not spheroidal. The bow-shaped filaments also present a dark straight aspect when they turn over (Fig. 5, *e*), and are by far the longest of the forms, some measuring as much as $\frac{1}{500}$th of an inch. Many intermediate forms between the round discoid corpuscles and those with long tails are met with in the field, and are figured at Fig. 5, *g*.

Small rod-shaped forms are very numerous, most of which, however, on one aspect look corpuscular; but in others this cannot be detected, or only with the greatest difficulty; slight enlargements at each end may also be seen occasionally in these forms (Fig. 5, *h*).

Usually late to appear, and more often seen in the profuse

* The mass from which this sketch was taken was seen in full development by several of the foreign visitors to the British Medical Association last year.

developments from large masses, are the forms with three or more tail-like processes attached to a small central body (Fig. 5, *k*). Among the granules it is extremely difficult to determine accurately the number of these processes, the apparent number of which may also vary in the different positions assumed by the element. As to the ultimate destiny of the individual forms, I have not much to offer; I have watched single ones, with this view, for several consecutive hours without noticing any material alteration in them. The one represented at Fig. 6 was watched for four hours, that at Fig. 7 for five, and the changes sketched. The difficulty of following up individual filaments in this way is very great, not only from the ensuing weariness, but from the obstacle the red corpuscles offer to it.

With regard to the movement of the filaments, this, at first sight, bears some resemblance to that known as the Brownian, exhibited by granules in the field, or sometimes by the red corpuscles; but an evident difference is soon noticed in the fact that, while the former (also the small corpuscles) undergo a change of place, the latter remain constant in one position, or vary but little.

Movements like those of the ordinary rod-shaped *Bacteria* are not exhibited by them.

Circumstances which influence the development.—In blood, without the addition of saline solution or serum, no change takes place in the masses even after prolonged warming. A temperature of about 37° C. is necessary for the process; none occurs at the ordinary temperature, with or without the addition of fluid. Fresh serum is the medium most favourable to the process, added in quantity equal to the amount of blood. Not every mass develops when placed under conditions apparently favourable; but for this no good reason can, at present, be offered.

Fig. 8 represents the corpuscles among the red ones while in the vessel; and, as is there seen, they appear somewhat more elliptical on the profile view, and more elongated, than in blood after withdrawal, but present the same disk-like surfaces when they roll over. On adding saline solution or serum, and warming the preparation, development proceeds, but not to such an extent as from the masses. The individual corpuscles become elongated, some tailed, and they move about in the vessel. At Fig. 9 they are seen in the vessel after three hours on the warm stage: the remarkable form seen at *a* was $\frac{1}{1300}$th of an inch in length, and had moved up from the opposite end of the vessel.

It must still be confessed, with Max Schultze, that we know nothing of the origin or destiny of these corpuscles; and once admit their existence as individual elements circulating in the blood, his suggestion, and Riess's assertion that the masses arise from the disintegration of white corpuscles, becomes quite untenable. We

must also confess the same ignorance of the reasons of their increase in disease; nor do we know at all what influence they may exert in the course of chronic affections.

Finally, as there is no evidence that these bodies are in organic continuity with any other recognized animal or vegetable form, or possess the power of reproduction, nothing can at present be said of their nature or of their relation to *Bacteria.*

These observations were carried on in the Physiological Laboratory of University College, and my thanks are due to Prof. Sanderson and Mr. Schäfer for advice and valuable assistance.—*A Paper read at the last meeting of the Royal Society.*

NEW BOOKS, WITH SHORT NOTICES.

The Micrographic Dictionary. A Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. By J. W. Griffith, M.D., M.R.C.P., and Arthur Henfrey, F.R.S., F.L.S. Third Edition. Edited by J. W. Griffith, M.D., M.R.C.P., and Professor Martin Duncan, M.B. Lond., F.R.S., F.G.S. Assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and T. Rupert Jones, F.R.S., F.G.S. Parts XI., XII., XIII., and XIV. London: Van Voorst, 1874. —Of the four parts of this Dictionary now upon our table, those numbered XI. and XII. have not been issued under the editorship of Dr. Martin Duncan, who has not appeared among the editorial staff till No. XIII. made its appearance about three months since. He must therefore not be held responsible for the earlier issue, though we believe he will bear almost entirely the weight of editorship for all the numbers which complete the work from and inclusive of the thirteenth part. Of the four parts now under notice it may be observed that but one of them possesses plates. This is the eleventh, which contains four pages of illustrations, many of them coloured, and most of them of the Infusoria. We have to observe, however, that these are, if we mistake not, exactly the same as those issued many years since, and they are therefore very far behind-hand. We not only refer to the size of the figures, which is on vastly too small a scale, but to the fact that our increased knowledge of the structure, due to the observations of Claparède and Lachmann, and of several English workers, especially Mr. E. Ray Lankester, is not portrayed as it certainly ought to be. But if these objections be accepted, it must be admitted that the numbers are in other respects very well executed.

It is, however, when we come to consider the matter in these four numbers that we perceive the great difference in many respects between Nos. XI. and XII., on the one hand, and Nos. XIII. and XIV. on the other. In the first place, we may remark that in the first two numbers the articles alone which have to do with either fungi or fossils are unquestionably good; most of the others are undeniably behind the time, having been left pretty nearly as they were when the last edition of the Dictionary made its appearance. Let us take two or three examples of the part of which we complain. The paragraph on the eye is by no means sufficiently full, even in regard to the microscopic subjects which the writer proposes to discuss. The question of the ciliary muscle, for instance, is almost untouched upon; and as to the question of the nerve supply of the cornea, which has been so fully dealt with even in these pages, the writer appears to have no knowledge of it whatever. And many other points might be alluded to. The article Fermentation, again, is vastly too brief; no allusion whatever is made to Pasteur's researches, though his name is mentioned in the bibliography appended to the paper; yet of course chemical readers are aware of the immense extent of Pasteur's researches, and of their bearing on some of Pouchet's inquiries in the same direction. Under the heading of Spontaneous Generation also

we observe a lamentable deficiency of information. This question has of late years had an amount of attention paid to it by Bastian, Sanderson, and others, which has given it a considerable interest to the microscopic observer. Therefore it is a subject which ought not to be dealt with as it was in the old edition, with merely the title of the "Beginnings of Life" added. Again, under the heading of Glands, some reference should have been made to the essays in the splendid treatise of Dr. Stricker, and for this absence the less excuse is to be offered, as the book has been for some years in an English dress. There are other articles of less importance, which, however, we shall not refer to.

Of the better parts of these two numbers we may particularly refer to the botanical portions, and especially to those of the numerous instances of fungi. These are, as we might have imagined, invariably lucid, to the point, and as advanced as possible. There are articles too of a geological character which are particularly well done. We may refer to the article Foraminifera, as one which is thoroughly well written and arranged. In this the author enters upon their general history, describes very fully their minute structure, tells where the recent and fossil forms are each to be found, and finally gives an ample synoptical list of the genera and sub-genera, with references to the figures of nearly all those enumerated. Finally, he appends an admirable bibliographical list of the various works necessary for consultation, from that of D'Orbigny in 1826, to those of Parker, Jones, and Brady, in the 'Transactions of the Linnean Society,' 1870.

Now, having said so much about the older numbers, let us examine what has been done for us in the two latest issues which have been brought out under the supervision of Dr. M. Duncan, F.R.S. We find in the first place the conclusion of the article Hydra, which is not quite as full as it might be, having taken little or no notice of the more recent Austrian inquiries. This article, too, seems somewhat hastily written, as the following sentence will show. "On placing the plants subsequently in a glass jar containing water, they will be found at the end of some hours with the tentacles fully extended in search of prey, when they are easily recognized." Of course we have no doubt that the author means the Hydra, but it is the plant to which he refers. The article on *Hydrodyction* is good, as is also the paper on *Hyphomycetes*, which is capitally illustrated. The section devoted to Illumination is extremely short, and though the editors may make up for it by increasing the length of Polarization and Test-objects, yet we think the subject of illumination should have been in itself more fully written; indeed, we think the writer would have done well not merely to have referred to one of Mr. Wenham's papers in this Journal, but to have gone fully into the whole subject, and to have given the results to his readers. The paper on Inflammation is, we are bound to say, in every respect worthy of its position. It is a condensed account of the entire subject, giving even the latest researches, and stating as much as possible facts to the exclusion of useless hypotheses; indeed, it is one of the best contributions to the present edition. Next in order come the Infusoria, and as these are perhaps the most important groups with which the Dictionary has to deal, no less than ten pages are

devoted to them. And certainly we think the work is very excellently done. Doubtless some will say it is not sufficiently minute, but we venture to think that an article which deals with the entire subject should avoid specialities and deal more with generalizations. And this is what we find in this contribution. The author first gives a general account, and then proceeds to deal with the integument in which the *outer cuticle*, the *carapace*, the *cortical layer*, and its peculiar thread-cells are minutely described; then the locomotive organs, under which heading are detailed the various forms, such as *cilia*, *flagelliform filaments*, *retracting cilia*, *setæ*, *styles*, and *uncini*; then the nervous system, under which are related the various indications of a nervous arrangement, although no traces of it have been yet distinguished; and likewise the circulating, the digestive, and the reproductive systems, especially the last, are minutely described. In the next place their diffusion is spoken of, and a very full account of the systematic classification is given. Under this latter head the two systems, those of M. Dujardin on the one hand, and of MM. Claparède and Lachmann on the other, are given, and finally the bibliography which concludes the paper is as full as needs be.

The next subject that we may refer to is that of Injection, which is by no means so novel as we should have anticipated. The part devoted to the opaque injection fluids is of course very good, but that which describes the different transparent liquids which are now almost exclusively used for preparations, is meagre and short; and in the references to books on the subject, one of the very best, as well as the cheapest books, viz. that of Davies, is, we observe, omitted. The description of the method of performing the operation is extremely fully given, and this is a point of some importance, for there is no subject in which the young microscopist is more liable to fall into errors. We observe that the authors have described an apparatus for injection which performs its own work, thus leaving the hands free for the purpose of stopping any escape of the fluid, &c. This may doubtless prove useful in some cases, and it can be readily put up by any ingenious person in the course of half-an-hour. Next in order we come to the Intestinal Canal, which has not been very much brought up to the time, although it is very good, and well illustrated. The bibliography, so far as it refers to Verson and E. Klein, is good, but the succeeding reference we do not quite understand, the more so, as the title given is really that which should have followed MM. Verson and Klein. The other papers in the thirteenth part to which we would call attention are those on *Isoëtes*, on the *Kidney*, on *Lagena*, on *Lar*, which is a little too brief, and on *Lepisma*, which, considering the importance of the Thysanura to the microscopist, is far too shortly given.

In the fourteenth part the best contributions are undoubtedly those on *Lichens* and on *Liverworts*. These are unquestionably well and fully executed, and further the reader has given him a succinct account of the more modern ideas regarding the supposed algoid and fungoid relationship of these plants, while in regard to their structure and development the subject is as recent as possible. Ligaments is a fair paper, not however extremely recent, and its bibliography, unlike that

of the last-mentioned two groups, is very short and imperfect. In the paper on the liver the text is indeed very good, and is amply illustrated by woodcuts. Still we think that had the author dwelt a little on the distinction between Handfield Jones's views and Beale's, and on the recent foreign development of the former's notions, he would have given additional interest to the subject. The lungs too are not badly done, but had there been more space given to the lymphatics we think it would have been better. However, this is more than made up for by the way in which the lymphatic system is described. In this article the author has dealt briefly, but yet clearly, with the entire subject, and he has entered on a discussion of the views recently put forward by Dr. Klein. Measurement, too, is not badly done, as likewise are "Micro-Spectroscope," "Migration of Cells," "Monera," "Motion of Cells," and lastly, "Mosses," which are given at some length, and with numerous illustrations. The bibliography of mosses is, however, insufficient, and is erroneous in the fact that the only reference to Dr. Braithwaite's numerous papers in these pages is to the 'Quarterly Microscopical Journal.' This should be corrected.

But it will be judged from what we have already stated that the last two numbers of the Dictionary exhibit a marked superiority of matter over those previously issued, and we doubt not that the parts which are yet to be published will be even better. In this way we may hope that the work will, as a whole, be worth purchasing; and if the publisher proposes adding to the plates and improving figures which require alteration, while the editors continue to supply new material, we have no doubt that a capital companion to the microscopist will be supplied. E. H. S.

Microscopic Examinations of Air. By D. Douglas Cunningham, M.B., attached to Sanitary Commissioner with Government of India. Calcutta: Superintendent of Government Printing. 1874.—In the work which Dr. Cunningham has published with the aid of the Government printer, we have a copious summary of the different views *pro* and *con* which have been held by those who have already considered the questions concerning atmospheric germs. This constitutes at least one-half of the volume, and as it is material already before our readers we need not do more than advert to it. The latter portion of the work contains the author's own observations, and is illustrated by fourteen nearly folio plates, exhibiting coloured illustrations of the different results obtained, magnified 400 diameters. The book is, of course, altogether most elaborately executed, and would have been utterly impossible to produce save as a Government matter, from the very great expense it must have involved. And, indeed, here we would blame Dr. Cunningham for extravagance, for it is perfectly clear to anyone who understands the matter that a single folio page of illustrations would have been ample, inasmuch as there is no possible difference between many of his drawings. With this, however, we have nothing to do. But of the actual value of the results achieved we cannot say much. In point of fact, the author has not been able to prove that any season of illness in Hindostan has had anything whatever to do with the presence of microscopic vegetable matters. Indeed, he has half

shown that one or two instances of cholera were connected with the presence of certain organisms in the air;, but on going into the matter he has been assured that this has not been the case.

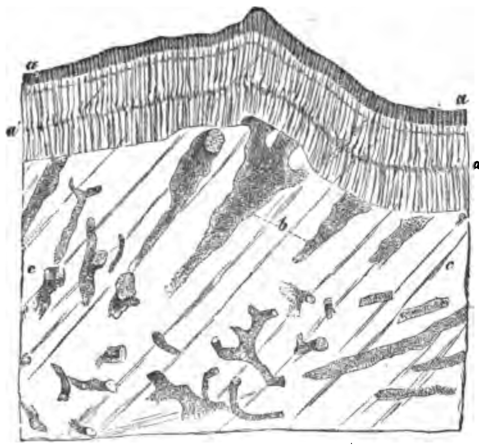
Dr. Cunningham has been at considerable pains in carrying out his experiments, and indeed on this subject we must say that he has taken every precaution to avoid failure. Especially would we refer to the excellent modification of Dr. Maddox's apparatus which he has employed in his experiments, for this is an instrument which may be used with excellent results in some future inquiries. We must also award very high praise to the author for the care and discretion which he has shown in conducting his experiments, and for the accuracy with which he has stated the results. Beyond this, however, we have little to say in his praise; for it seems to us that he has gone over ground that ought to have been pregnant with valuable results if the experimenter had employed the proper means of research, and those we think Dr. Cunningham failed to apply. Why, for instance, should he have used such low powers in his observations? Surely he did not expect to find many organisms with objectives magnifying 400 diameters; the microscopist of the present day would not be so well employed with such a magnifying power as Leüwenhoek was with his lens. And so far as we can see, the author, save in one or two instances, used no higher object-glass. How he could expect to see everything that was present with such amplification is to us perfectly unintelligible. If Mr. Dallinger had had Dr. Cunningham's opportunities, we doubt not he would have found many organisms, as we may judge by his published papers. So far as he has gone, Dr. Cunningham has nothing to tell. But may we not say to him, look again? With a $\frac{1}{25}$ or $\frac{1}{50}$, and proper arrangements for light, we may expect much better results than those he has laid before us in his present by no means uninteresting volume. J. D.

PROGRESS OF MICROSCOPICAL SCIENCE.

Dr. Carpenter's Views on the Subject of Eozoon.—In our last number but one we gave ample space toward an explanation of Mr. Carter's opinions on the subject of Eozoon; and it will be remembered that he takes the same view of this structure as that held by Messrs. King and Rowney. We have now the opportunity of laying Dr. Carpenter's views forward, accompanied by an excellent woodcut of one of the calcareous lamellæ of Eozoon, which the editor of 'Nature' has kindly placed at our disposal. In Dr. Carpenter's recent paper in the 'Annals of Natural History' for June (1874), the subject is very fully gone into, and to that we must refer those of our readers who are interested in this question. However, with the aid of the woodcut we may extract some of the author's remarks which bear on the point of controversy. Dr. Carpenter says—

"My *true* 'nummuline wall' is the representative of that which,

in recent Foraminifera, immediately surrounds the chambers (see Fig. *a a*). It is *not* a layer of chrysotile aciculæ, as asserted by Professors King and Rowney, but is a *calcareous* lamella, perforated by



This represents a vertical section of a portion of one of the calcareous lamellæ of *Eozoon canadense*, showing the tubular "nummuline layer" *a a*, the "intermediate skeleton" *c c*, and the relations of the origin of the canals *b b* to the tubuli of the nummuline layer, the flexures of which are seen along the line *a′ a′* : 100 diameters.

minute tubuli, which usually lie straight and parallel, but are often more or less curved. These tubuli, like the chambers and canal-system, are usually filled with serpentine, which has passed into them from the chambers in which they originate; and thus it happens that the original tubulation is generally obscured, being only represented microscopically by the difference in refractive index between the calcareous shelly layer and the serpentine which has filled its tubes,—just as in a specimen of fresh bone or dentine mounted in Canada balsam, the tubuli are only represented by the different refractive indices of the matrix and the balsam. But in the specimen of *Eozoon* figured above, many of the tubuli remain empty; and *they can be distinguished as tubuli under any magnifying power that the thickness of the covering glass allows to be used.* Further, they have the somewhat sinuous course of the tubuli of organic structures; and they present, at what was probably a plane of interrupted growth (*a′ a′*), the sharp flexures which Professor Owen first pointed out in the tubuli of dentine, and which I described and figured twenty-seven years ago in the hard dentine-like substance of the end of the Crab's claw."*

And after some further remarks *à propos* of the histological powers of his opponents, Dr. Carpenter says:—

"I now pass on to a *second* probative fact of at least equal cogency,—the relation exhibited in the same specimen between the 'canal-system' and the tubuli of the 'nummuline layer.'

* Report of the British Association for 1847, pl. xx., fig. 81.

"In my original description of *Calcarina* *—the type to which, as regards the general distribution of its canal-system and its relation to the intermediate skeleton, *Eozoon* has the closest resemblance—I gave the following account of that relation (p. 554): 'The proper walls of the chambers are uniformly perforated, like those of the chambers of *Rotaliæ*, by foramina of considerable size (averaging above $\frac{1}{3000}$th of an inch in diameter); with these the canals of the supplemental [or intermediate] skeleton do not seem to be directly continuous, for they are of about double the diameter and lie further apart from one another; but immediately round the proper walls of the chambers there seem to be irregular lacunar spaces, into which the foramina open externally, and from which the passages of the canal-system originate.' Now, in my 'Supplemental Notes on the Structure and Affinities of *Eozoon canadense*'† I stated that precisely the same relation is shown to exist in decalcified specimens of *Eozoon*, by the implantation of the dendritic models of the chamber-casts in plates formed by the coalescence of the aciculæ that occupied the tubules of the 'proper wall.' Having now been fortunate enough to meet with a *transparent section* which exhibits this relation most unmistakably (see Fig. *b b*), I fearlessly ask the verdict of any Biologist familiar with microscopic structure, whether any more exact realization *could* be presented of the structure I had described in *Calcarina*,—allowance being of course made for the different scale of the tubulation of the 'proper wall,' which is here *fine* 'nummuline' not *coarse* 'rotaline.'"

We think the verdict of most microscopists will be to the effect that the structure is unquestionably one of organic origin.

Retrogressive Changes in the Serous Layer of the Rabbit's Ovum.—Dr. Stirling says that Herr K. Slavjansky‡ describes the degeneration, called by him reticular ("reticulare degeneration"), which the epithelial cells of the serous layer of the ovum undergo in their physiological development. During the development of the ovum, the epithelial cells of the part of the serous layer lying close to the umbilical sac become thin and flat, and in the cells themselves some transparent spots are to be observed. By-and-by the protoplasm disappears, and holes are observed in the cells. These holes gradually enlarge, so that, at last, in place of the epithelial membrane there is to be seen a reticulum of the remains of the protoplasm of the epithelial cells, containing in some places the nuclei. There is thus established a physiological prototype for the pathological degeneration of the epithelium, described by Wagner under the name of fibrinous degeneration, in cases of croup and diphtheria.—See also 'Medical Record.'

The Microscopic Blood-vessels of the Intestine.—In a note in the 'Medical Record,' by Dr. Stirling, the writer says that Herr A. Heller§ arrives at the following results:—1. Every villus contains an artery which runs, as a general rule, to the point of the villus without branching. In man only does it begin from the middle of the villus

* 'Phil. Trans.,' 1860. † 'Proceed. Geol. Soc.,' Jan. 10, 1866, p. 222.
‡ Ludwig's 'Arbeiten,' vol. vii. § Ibid.

to lose itself in a capillary network. 2. The vein begins either in the point of the villus (rabbit, man) or near to the same (rat), and generally goes directly into the submucous tissues without receiving any lateral branches; or it rises near the base of the villus and receives more or less numerous lateral branches from the glandular layer (dog, cat, pig, hedgehog). 3. In none of the animals examined was there to be found the often cited arrangement of an arterial stem going to the point of the villus, and of a descending venous stem with a simple connecting capillary network between both stems. This is of importance with regard to the erection of the villus.

Microscopic Structure of the Cortical and Corky Tissue of Plants.—Dr. Braithwaite, F.L.S., is now publishing, in the 'Journal of the Quekett Club,' a series of lectures on vegetable histology, which are of great interest. From a proof sheet of the last number of that Journal which he has sent to us, we abstract the following account of the structure of cortical tissue and cork, which are two of the group of homogeneous or purely cellular tissues. He says of the first, that it includes that portion of the stem lying between the fibro-vascular bundles and the epidermis or cork, and in leaves between the cuticle and vascular bundles of the nerves; it is therefore most distinct in parts exposed to the air and light. The primary bark proceeds directly from the primordial tissue of the growing point, and rapidly increases by cell division. In annual plants it is completed simply by extension of these cells, as it is also in perennial plants which cast off their bark by cork tissue arising under it; but in those like the holly and the mistletoe, which do not do so, certain portions of the cortical tissue, by mother cells, continue to reproduce new tissue of the same kind.

Cortical tissue consists entirely of parenchym cells, which in leaves usually remain with thin walls, but in the stem are variously modified and may be divided into an inner and outer rind.

The inner rind is formed of layers of thin-walled spheroidal cells, with their surfaces only slightly in contact, and thus interrupted by apertures of various sizes. Lignification of the cellulose case very rarely occurs, but in a few instances groups of strongly thickened cells are seen, distinguishable by their size and colourless contents; the ash, beech, laburnum, and *Hoya* afford examples. The contents of the cells of this layer are starch, with the addition of chlorophyll in the more external, and in some instances crystals are also present. Again in milky-juiced plants bast vessels occur, which are connected with similar vessels of the bast bundle, and single and grouped bast cells are seen in the inner cortical layer of the leaf-stalk of cycads and the bark of many palms. Within this layer also occur the resin, oil, and gum canals peculiar to many plants.

The outer rind, Collenchyma.—The outer layer consists of rounded parenchyma cells with little or no thickening, but often more or less elongated, or the cells have all irregular strong thickening of their walls or angles, and then constitute *collenchyma.* When the outer layer of this sub-epidermal tissue consists of thin-walled parenchyma and collenchym cells, the latter are in groups overlying the bast part of the vascular bundle, while the thin-walled cells reach the epidermis,

and are opposite the medullary rays; in these cases the collenchyma is often greatly elongated. The collenchyma has no intercellular spaces, and may take the form of longitudinal strings of cells lying under the epidermis, as in the stem of *Equisetum* and leaves of *Pinus;* or it may be seen as a connected layer, only perforated by the stomata, in the stems and petioles of many plants, and also in many leaves as a well-developed layer, e. g. in the vine, elder, and begonia. The cellulose case is usually soft, but in a few instances lignified, as in *Angelica sylvestris*, and in others shows porose, netted and spiral thickening, e. g. *Sambucus*, *Helleborus*. The contents are clear or red sap, and also starch and chlorophyll.

Of the second, or Cork Tissue, he says, that it is of much more frequent occurrence in plants than may be generally supposed, and moreover it is Dame Nature's plaster with which she heals up the wounds left by fallen leaves, or if any soft organ be injured, a firm skin of new cork cells rapidly protects the sound tissues from the outer damaged structures. The walls of this tissue are highly resistent to the various reagents, behaving in this respect like the cuticle, being also elastic and with difficulty permeated by air or water; the cells are rectangular without intercellular spaces, are arranged in rows at right angles to the surface, and mostly lose their contents and become filled with air; the cell membrane is but moderately thickened, and is soon altered into cork. Primary cork tissue arises later than the other elements, and the altered parenchym cells, which become the mother cells of cork, may be either cells of the cuticle, of the collenchyma, of the inner rind, or of the parenchyma of the bast part of the vascular bundle; these mother cells repeatedly divide, and of these newly-arising cells in each radial series, the inner one remains thin-walled, filled with protoplasm, and constantly forming new cells by division, and this is termed the *cork—cambium* or *phellogen layer*, while the outer becomes suberified and permanent. Generally the cork first commences at single points, but these gradually coalesce, and the phellogen forms a continuous layer, from which constantly new cork layers are being pushed outward and constitute the *periderm.* Sometimes the cork cells become altered in form, and the periderm consists of alternate laminæ of different shaped cells; this is seen in the cork of the cork-oak, and of birch. As examples of cuticular development of cork we may mention the apple tree, oleander, mountain-ash and *Viburnum Lantana;* here the epidermal cells divide into two daughter cells, the upper of which with the cuticular layers and tertiary cellulose case become suberified, and the lower becomes the mother cell of the next cork formation. In the greater part of our trees, as in the maple, beech, oak, elm, plum, horse-chestnut, elder, &c., the collenchym cells lying next under the cuticle become the mother cells of cork; and among the number of plants in which the cork tissue arises deeper below the cuticle, but yet within the outer rind, *Ficus elastica* and *Robinia pseudacacia* are well suited for observation; here the cells of the second or third row of collenchyma become the mother cells of the cork. In the bramble and currant bushes the cork tissue arises in the inner rind, and indeed it is the cells next to the vascular

bundle which become the mother cells, so that all the young cortical tissue becomes pushed off by the cork tissue.

In many cases it is not solely cork cells proceeding from the phellogen which give the thickness to the periderm, but parenchym cells containing chlorophyll are also formed; these, however, are always the daughter cells of the phellogen lying on the inner side which become thus metamorphosed, and constitute what Sanio terms the *Phelloderma*, very well seen in the currant tree.

Bark.—After production of more or less numerous cork lamellæ, the phellogen dies or loses its vital activity, but a development of secondary cork tissue takes place within the bast part of the vascular bundle, in the form of tangential rows of tabular cork cells, which loosen from the growing outer part of the vascular bundle. The cork lamellæ, as it were, cut out and force off from the rind, flat pieces in form of scales or rings; all this outer part is dead, and the process oft repeated from the circumference of the stem, causes the new cork lamellæ to become gradually imbedded more deeply in the growing cortical tissue, and we get a constantly thickening peripheral layer of dry tissue separating from the living part of the rind; this is the bark. The condition is very evident in the large scales of bark in *Platanus orientalis* or sycamore, and in old stems of the *Pinus sylvestris* or Scotch pine, and in the ring-like bands of the cherry tree. In the oak, lime, poplar, elder, and horse-chestnut, similar plates of thin-walled cells arise in the interior of the bast bundles, but the old dried scales do not fall off, but tear only at the margins in a longitudinal direction, so that the stem becomes clothed with bark consisting of several dead scales lying under each other, presenting internally all the elements of bast, and externally primary cork tissue. In the pine and larch we have a fissured periderm, like that of the horse-chestnut, and in the pine consisting partly of thin-walled and thickened cells in alternate layers, but the conifers are specially remarkable for the presence of a spurious large-celled parenchym tissue, which appears between the periderm layers and separates the elements of the bast bundle into smaller or larger groups.

Lenticels.—These are due to a peculiar local cork formation, and appear as little roundish spots on the annual shoots of trees, while the epidermis continues uninjured, and before the periderm is formed. In the second summer the epidermis splits longitudinally over the lenticels, and they form more or less prominent warts, which by a median furrow frequently become bilabiate; their surface is mostly brown, and their substance to a certain depth dry and corky. By growth in thickness of the shoot, the lenticels become expanded into transverse striæ, then cork or bark forms and splits the rind beneath them, the bark scales off, and so they disappear. These and other structures are very well illustrated in a plate which accompanies the paper.

NOTES AND MEMORANDA.

Resolution of Amphipleura pellucida by the $\frac{1}{50}$ of Mr. Tolles.—The 'American Naturalist' for July, 1874, contains a note by Mr. G. W. Morehouse on the above subject. A $\frac{1}{50}$ objective was made for him by Tolles, and finished on the 12th of March, 1873. The angle of aperture as invoiced by Mr. Stodder is 165°. From his own measurements he thinks the objective is correctly named by the maker. At the extreme open point it is a good $\frac{1}{40}$th dry. The screw-collar has twelve divisions; by turning it eight divisions it is adjusted for uncovered wet, and four divisions remain to adjust for cover for immersion work. It works through covering glass of about $\frac{1}{200}$th of an inch, but it is better to use thinner glass, or mica, to enable the observer to focus through specimens. With lamplight and the $\frac{1}{50}$th the resolution of *Amphipleura pellucida* is better than he has before seen. Using ordinary daylight, vibriones, bacteria, &c., are well defined, especially when a Kelner eye-piece is used as a condenser. With sunlight and the ammonia-sulphate of copper cell, *Surirella gemma* yields longitudinal striæ, and, as the direction of the light is changed, rows of "hemispherical bosses" as described by Dr. Woodward. With the same illumination specimens of *Amphipleura pellucida*, mounted dry, by Norman, were resolved and counted with perfect ease and remarkable plainness, the striæ being still distinctly visible with No. 3 eye-piece, draw-tube extended six inches, and power upward of 10,000 times. It is with hesitation that he remarks further that the $\frac{1}{50}$th has resolved the lines of *Amphipleura pellucida* into rows of dots, for the "beaded" structure of the easier test, *Surirella gemma*, is still doubted by some experienced microscopists. But "facts are stubborn things," and the facts are that with Wenham's parabola as an illuminator the dots are seen, and with either the paraboloid or the Amici prism longitudinal lines much finer than the transverse ones are brought out. These lines, which he considers genuine, count not far from 120,000 to the inch. With a slight change of the adjustment their place is occupied by spurious lines counting generally about 60,000 to the inch. The longitudinal lines can only be seen when the focus is best adjusted for the transverse striæ. When the transverse lines are examined, they may be shown smooth and shining, similar to the photograph by Dr. Woodward in the 'American Naturalist,' but much better. If the mirror is then carefully touched, a sinuate appearance of the margins of the lines suggestive of beading is seen. This appearance can be brought out readily. And finally, after the most painstaking manipulation, and when without doubt the best work is being done, the separated dots or beads appear.

A Memoir on the Cyamus or Whale-louse has been published in the 'Memoirs of the Scientific Society of Copenhagen,' by Dr. Lütken. We believe it is an interesting paper, but we have not yet seen it.

Diatoms on the Surface of the Sea, from Java and also from the Arctic Sea, have been very admirably figured and described in English by M. P. T. Cleve, in a couple of papers, which were originally presented to the Swedish Academy of Sciences last year. With them has come to us a paper in Swedish, also read before the Swedish Academy of Sciences about the same time, by M. N. G. W. Lagerstedt, on the "Diatoms of Spitzbergen." This is illustrated by two good plates, and though the general observations are in Swedish, the description of the species found is in Latin.

CORRESPONDENCE.

An Error in Mr. Morehouse's Paper.

To the Editor of the 'Monthly Microscopical Journal.'

Ashtabula, Ohio, U.S.A., *July* 17, 1874.

Sir,—In the article on the "Structure of Diatoms," by Mr. Geo. W. Morehouse, reprinted in your June number, I notice an error which occurred in the original text.

The last footnote, p. 23, should read "*J.* E. Smith," &c.

As many of your readers may not have access to the 'Lens,' I desire to say that the observations referred to were made by me in January, 1873; the objective used was a superb Tolles' immersion one-tenth ($\frac{1}{10}$th)—amplification about 4000 diameters.

These observations were very shortly afterwards confirmed by Mr. Morehouse.

I remain, Sir, your obedient servant,

J. Edwards Smith.

The Pathological Anatomy of the Brain and Dr. Kempster.

To the Editor of the 'Monthly Microscopical Journal.'

Royal College of Physicians, Edinburgh, *August* 18, 1874.

Sir,—I have this moment seen your notice of Dr. Kempster's observations on the pathological anatomy of the brain in the insane; in it, it is said that Dr. Kempster has only been able to find one exception to the silence which physicians have maintained on this subject, and that *one* paper by me is the exception. Were Dr. Kempster to search with anything like care, he would find that there have been many workers in the field both in Great Britain and the Continent, that my contributions are numerous, and that his own observations have been anticipated by many men and many years.

I am, Sir, yours truly,

J. Batty Tuke, M.D., F.R.S.E.

PROCEEDINGS OF SOCIETIES.

Medical Microscopical Society.

Friday, July 17, 1874.—Jabez Hogg, Esq., President, in the chair.

Skin Grafting.—Mr. Golding Bird read a paper on the mode of growth of the new epithelium after skin-grafting, or at the edge of a skinning ulcer. Specimens illustrative of the subject were exhibited. A summary of the changes observed is as follows:—A prolongation of the epithelium forming the rete mucosum of the adjoining skin, in a horizontal direction over the surface of the neighbouring granulation tissue, the vertically placed cells of the rete mucosum losing their upright position and becoming more and more inclined till quite horizontal; the epithelial scales placed more superficially taking no part in the process, but becoming shed; so that the new epidermis was only one-third the thickness of that of the skin from which it had sprung. He ascribed the adhesion of the new epidermis to the underlying granulation tissue to the insertion of the former into the most superficial layer of the latter, the intercellular material of which may be seen becoming fibrillated (like the fibrin of blood clot), coincidently with the growth onwards of the epithelium, the granulation cells disappearing in great numbers at the same time. He had never yet been able to find the granulation cells becoming developed into epithelium; but he had seen a few of them lying between the cells of the new epidermis. The granulation tissue beneath the earliest formed epithelium was the first to become developed into fibrous tissue.

Mr. Coupland thought the disappearance to the naked eye at times of a graft, and the subsequent growth of epidermis at the spot grafted some time after, was a proof of the development of epithelium from granulations.

Mr. Schäfer referred to the observed transformation of white blood-corpuscles on the recently blistered surface in the frog.

Mr. Golding Bird, in reply, denied that a graft that reappeared as stated had ever in reality disappeared. He believed that the deepest layer of epithelial cells was always left, though not visible to the naked eye.

Paccinian Corpuscles.—Mr. Schäfer gave an account of these bodies, discussing generally the various opinions held regarding them. He explained the various component parts, and held that the "core" was the layer of protoplasm described by Ranvier as covering the medullary sheath of the nerves. He has seen a nerve pass from one Paccinian body to another.

Dr. Pritchard asked if the Paccinian bodies in the cat's mesentery were the same as in the skin? In reply, Mr. Schäfer stated he considered them identical.

Mycetoma.—Microscopic specimens of the "Fungus-foot of India" were exhibited by the President.

there were mussels, a large number of the common barnacle and balanus, but none of the *Lepas anatifera* (the object of ancient fable). But the animals most abounding were ascidians, singular creatures, interesting to microscopists on account of the contents of the stomachs. Besides these were actiniæ, tubicolar worms, the larvæ of cephalopods, with various zoophytes, but few algæ. He had some of them now under the instrument, and hoped to find in them material for a future paper. He afterwards described the structure of some new and curious fungi, and the characters by which it was proposed to classify them, and he exhibited specimens under a powerful monocular microscope. He also communicated some notes on the rye-grass fungus, on which Mr. Ralph and himself had reported at a previous meeting, both agreeing in the view that it was deficient of some of the characters required to constitute it a clavaria. He intimated that its place had now been settled by Mr. Bentham and Baron Von Mueller among the Isariæ, with the name of *Isariæ graminiperdæ.*

The Chairman remarked that the subject was a very interesting one.

Mr. Sydney Gibbons next exhibited a section of the tooth of a rare animal, the orycteropus, a burrowing animal of the anteater family, whose dentition differed from that of all other known animals. The creature is devoid of canines and incisors, but has a double set of molars, one of which it sheds every year. To add to this anomaly, the structure of the teeth is unique, being composed of a cluster of pentagonal prisms, instead of the usual enamel with a bony foundation.

The meeting afterwards spent some time in examining the several specimens brought by members.


1
2
3
4
5
6.
7.
7
8.
9.
10.
11.
12.

THE

MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1874.

I.—*The Hairs of Caterpillars.* By T. W. Wonfor (Hon. Sec. Brighton and Sussex Natural History Society).

(*Read before the* Brighton and Sussex Natural History Society, *July* 23, 1874.)

Plate LXXV.

If either works on the microscope or on entomology be consulted, very little, if any, information can be obtained on the hairs of caterpillars or larvæ either as regards their structure or the variety and beauty of their forms, but here and there a few words may be found upon the urticating or stinging properties of the hairs of some caterpillars.

This urticating property was noticed in very early days, for, according to Pliny,* the Cornelian law, *De Sicariis*, was extended to those persons who administered the hairs of the fir moth, *Cn. Pityocampa*, and which were supposed to be a very deleterious poison. Even when applied externally they occasioned a very intense degree of pain, itching, fever, and restlessness.

Occasionally allusion is made to other members of the same family as possessing similar disagreeable effects, viz. *Cn. pinivora*, stone pine moth, and *Cn. processionea*, the processionary moth, the last named so called on account of the habits of the larvæ, when moving in the evening in search of their food. One caterpillar leads the way, followed for some two feet by single caterpillars in Indian file, then come ranks in twos, succeeded, at about the

EXPLANATION OF PLATE LXXV.

Fig. 1.—Garden tiger. *C. caja.* × 120.
„ 2.—Hop dog. *O. pudibunda.* × 120.
„ 3.—Oak eggar. *B. quercus.* × 120.
„ 4.—Lappet. *L. quercifolia.* × 120.
„ 5.—Satin. *L. salicis.* × 120.
„ 6.—Drinker. *O. potatoria.* × 120.
„ 7.—Brown tail. *L. Chrysorrhœa.* × 210.
„ 8.—Vapourer. *O. antiqua.* × 120.
„ 9.—White plume. × 120.
„ 10.—Gipsy. *L. dispar.* × 120.
„ 11.—Lackey. *E. lanestris.* × 120.
„ 12.—Sycamore tussock. *A. aceris.* × 120.

* 'Hist. Nat.,' I., xxxviii., cap. 9.

same distance, by threes, fours, fives, &c., until the main body advances twenty abreast, in so orderly and compact a manner, that no human army could move with greater regularity or be more obedient to the word of command. As soon as the leading caterpillar stops, the whole army halts; when he advances, they advance, until a fresh pasturage has been found, then they all disperse, until some signal calls them all together again.

Woe betide the luckless individual who approaches them while on the march, or incautiously handles them, for the tufts of short hairs, with which they are covered, possess the power of producing an inflammatory irritation, worse even than the sting of a nettle. It is reported that in some cases, where persons have been stung severely, serious and sometimes fatal illnesses have resulted.

Not only when living, but even when dead, the hairs of this caterpillar possess the same urticating properties. Thus Reamur, who has written a monograph on this moth, states that he suffered, after handling the dead caterpillar, for days with an itching, in consequence of some of the short stiff hairs sticking in his skin, and being, at first, ignorant of the cause, and rubbing his eyes with his hands, he brought on such a swelling of the eyelids that he could scarcely open them. Bonnet, too, who lifted some of these caterpillars from water in which they had been drowned, felt a numbness of the fingers, followed by an itching and burning sensation.

Fortunately or unfortunately, I have not come across this caterpillar, which abounds in France, and in 1865 was the cause of so much annoyance to promenaders in the neighbourhood of Paris that parts of the Bois de Boulogne were closed to prevent discomfort to those who incautiously approached the trees, where the larvæ were, so that I only know the hairs from published drawings of them.

We have in this country several caterpillars whose hairs produce the same or similar effects with some people, and as these hairs present microscopically diversity of form I will specially direct attention to them. Among the most notable are *L. quercifolia*, the lappet; *O. potatoria*, the drinker; *B. neustria*, the lackey; *B. quercus*, oak eggar; *E. lanestris*, small eggar; *O. pudibunda*, hop dog; *O. antiqua*, vapourer; *L. dispar*, the gipsy; *L. salicis*, the satin; *L. auriflua* and *L. chrysorrhæa*, gold and brown tails; together with *C. caja*, and *C. villica*, the garden and the cream spot tiger. All these with some persons produce, when handled, either in the living or dead state, itching, inflammation, and swelling of the parts affected for days.

There is one very extraordinary fact connected with these hairs, viz. that while some are affected even if only the fingers touch the hairs, others can handle some with impunity, and cannot come near others without experiencing discomfort. I have known cases where

even the most careful handling of the brown tail has caused pain, and the person so affected, incautiously like Reamur, rubbing the face, could scarcely see out of his eyes for days after. When the hairs of this caterpillar are examined under the microscope the wonder ceases, as it will be seen they are admirably adapted to penetrate, whichever end touches the skin, while the jagged portion, barbed like an arrow, remains firmly fixed in the wound.

The typical form of hair among caterpillars is cylindrical and terminating in a sharp point; the hair itself being composed of the same chitonous substance as the skin of the animal, generally hollow and lined with a substance, which seems to resemble *cutis*. Many, if not all, hairs, in the living state, contain fluid matter, possibly of the same nature as the circulatory fluid of the animal. Instead of springing from a bulb, as in mammals, the base of the hair is inserted in a socket, a ring-shaped projection, from which the hair easily parts company. Examples of simple hairs of this character may be obtained from the larvæ of the oak eggar and lappet, both of which irritate some persons. The larvæ in each case utilize their hairs in forming their cocoons, as is often painfully evident to some, when handling them. A member of my family cannot touch a cocoon of the oak eggar, however old it may be, without annoyance, while I can handle them with impunity.

In the case of the garden tiger, hop dog, and some others, the hairs are deeply spinous from point to base. In the satin, sycamore tussock, and some others, the spines are thickly studded along the whole hair. In the case of the gipsy, the drinker, and the lackey, all of which, and especially the last named, punish some very severely, the hairs are very fine and beset throughout their length by very minute spines. In the brown tail, among longer spinous hairs, are immense numbers of very minute ones jointed throughout their length, and readily separating into barbs sharply pointed at one end and trifid at the other. These hairs part from the caterpillar so readily that persons looking at them, while they were feeding, have felt annoyance, as though the mere movement of the caterpillar separated the hairs, which, like those of the processionary moth, were wafted by the wind. Some very peculiar hairs are found on the vapourer, knobbed and plumed at the end; a similar, but more extensive knob is seen on the hairs of a South American caterpillar. The hairs on the tortoiseshell and other Vanessidæ are very stout and jointed, while those from the white-plume moth caterpillar are imbricated and have somewhat the appearance of wool: sufficient has been said to show there is so great variety and beauty among caterpillar hairs as to recommend them to the notice of microscopists, who have simply studied, as far as I can gather, those from the larvæ of Dermestes and the pencil tail, which are well known as test objects of great beauty.

A question may arise, Whence the urticating power? In the hair alone, or some irritating substance within the hair? I am inclined to the former, because hairs from cast skins kept for years, and from cocoons two or three years old, are equally urticating with those from a living caterpillar, as are also the hairs mingled with the webs spun by some of the sociable larvæ. I look upon it as a merely mechanical action, similar to that produced by the hairs of the prickly pear, those from the interior of the fruit of the wild rose or *Cowhage, Dolichos urens,* all of which are equally productive of irritation, inflammation, and feverishness.

To make out their structure caterpillar hairs should be mounted dry, in fluid and in balsam. Anyone turning his attention to them and not minding the risk of an occasional annoyance will be well rewarded for his *pains*, and possibly wonder why more attention has not been paid by microscopists to so interesting and instructive a class of objects. I can only account for the apparent neglect on the ground that few entomologists work with the microscope, and that microscopists generally have thought the hairs of all caterpillars alike, whereas, as with the scales of the lepidoptera, so with the hairs of their larvæ, there is great variety of form and markings.

Finding so little said about them, and having moreover worked at them for some years, I considered the subject of sufficient interest to bring before the Society, with the hope that some members may be induced to carry it further than I have done at present, and to show to our lepidopterists that there is much in the economy and philosophy of their branch of study worthy of being critically examined.

II.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Plates LXXVI. and LXXVII.

16. Sphagnum Lindbergii Schimper.

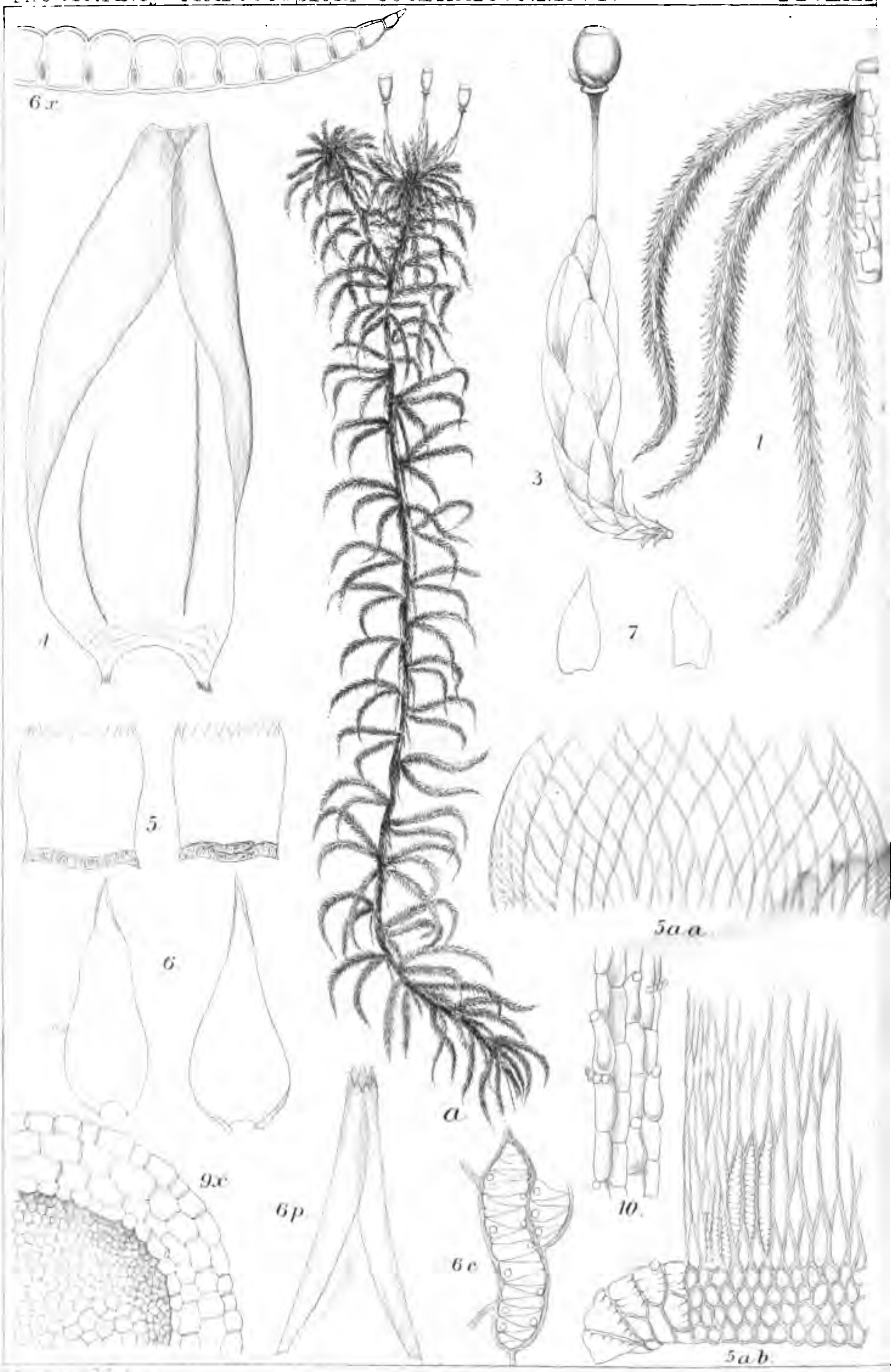
Torfmoose, p. 67, Tab. XXV. (1858).

Plate LXXVI.

Syn.—Schimper Synop. p. 679 (1860).—Lindberg Torfm. No. 2 (1862).—Hartm. Skand. Fl. p. 81 (1864).—Russow Torfm. p. 54 (1864).—Milde Bryol. Siles. p. 389 (1869).—Austin Musc. Appalach. No. 40 (1870).

Sph. cuspidatum β fulvum and *Sph. fulvum* Sendtner Mss. *Sph. cuspidatum* Lindberg in Bot. Notiser 1856, p. 122.

Monoicous, in large dense tufts 6–12 in. high, *glossy yellowish green, tinged with ferruginous or purplish brown. Stem solid, dark brown, with 3–4 cortical strata formed of irregular sized cells without pores. Cauline leaves crowded, reflexed, broadly lingulate, auricled, the apex broad, truncate and fringed;* basal cells hexagonal, in four rows, pale brown, then becoming narrow and



W. West & Co. lith

Sph. Lindbergii

elongated, with a few imperfect fibres in the lateral cells, these bound a central triangle, the base of which is formed by the apical margin, and this space is occupied by large loose rhombic cells, broader and 2–3 partite at the apex of leaf; both fibres and pores occur sparingly in the auricles.

Fascicles of 4–5 *branches, of which* 2–3 *are arcuate and divergent, the others pendent, elongated and closely appressed to stem.* Retort cells of the branches larger, recurved at apex.

Branch leaves numerous, in five rows, not undulated, firm, brownish or ferruginous green, rather glossy, ovate at base becoming lanceolate above, toothed and involute at apex; hyaline cells elongated, with numerous annular and spiral fibres, and many minute pores at margin; chlorophyll cells narrow, elliptic in section, quite enclosed but nearest to the back of leaf; border widest at base, formed of 3–4 *rows of very narrow cells.*

Male inflorescence consisting of few antheridia which are borne on the pendent branches.

Capsules numerous, seated in the capitulum, moderately elevated; *perichætium large, inflated,* the bracts yellowish green, lower elongated oblong, upper broadly obovate—oblong, convolute, truncate and fimbriate at apex, transversely undulate at base, without fibres or pores. Spores yellow.

Hab.—Deep bogs in the northern region of Europe. Discovered by Lindberg in 1856 near Lakes Betsetjaur, Skutijaur and Storavaviken in Pitean Lapland, and since found to be pretty generally distributed in other parts of Lapland as well as in Finland and the north of Sweden; Dovrefjeld, Norway (Berggren); in the Riesengebirge, Silesia (Milde); Alps of Salzburg (Sauter). In this country it was found in 1867 by McKinlay on Ben Wyvis in Ross-shire, and in America it has been met with in Canada, Newfoundland, and Greenland. Fr. July.

This fine species closely resembles *Sph. intermedium,* but is readily known by the different form of the stem leaves, and the non-undulated branch leaves unaltered by drying, as well as by the glossy reddish brown colour. The species also appears to be subject to hardly any variation, and will doubtless be found in other localities in the north of Scotland.

EXPLANATION OF PLATE LXXVI.

Sphagnum Lindbergii.

a.—Fertile plant.
1.—Part of stem and branch fascicle.
3.—Fruit and perichætium. 4.—Bract from same.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same × 60. 5 *a b.*—Ditto of basal wing.
6.—Leaves from middle of a divergent branch. 6 *p.*—Point of same. 6 *x.*—Transverse section. 6 *c.*—Cell from middle × 200.
7.—Basal intermediate leaf.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

17. Sphagnum Wulfii Girgensohn.

Archiv für Naturkunde Liv—, Est— und Kurlands, Ser. 2, Band II., p. 173 (1860).

PLATE LXXVII.

Syn.—*Sph. Wulfianum* GIRGENSOHN l. c.—Bot. Zeit. 1862, p. 247.—RUSSOW Torfm. p. 66 (1864).—MILDE Bryol. Siles. p. 385 (1869).

AUSTIN Musc. Appalach. No. 32 (1870). *Sph. pycnocladum* ÅNGSTROM Mss. RABENH. Bryoth. Eur. fasc. XV, No. 709.

Monoicous. Plants robust, 5–10 in. high, yellowish or brownish green or sometimes deep green, in loosely cohering tufts. *Stem simple*, or sometimes divided, *blackish brown*, straight, solid, *densely ramulose, with two cortical strata of small non-porose cells, the woody zone purple*, of 5–6 rows of strongly thickened cells. *Ramuli* 7–12 *in each fascicle, of which* 3–5 *are divergent, short, slightly arched, becoming clavate upward and then suddenly pointed;* the rest deflexed and closely appressed to stem, very long and slender, lax-leaved, usually of a pale rose colour; the porose cortical cells short and scarcely differing from the rest. *Branches of the coma short, thick, numerous, forming a large dense capitulum.*

Cauline leaves very small, from a broad base lingulate-triangular, reflexed; the hyaline cells repeatedly divided, without fibres or pores, those in the middle rhomboidal, becoming narrower towards the margin, where they form a border of 3–6 rows.

Ramuline leaves imbricated, erecto-patent, recurved in their upper half or subsquarrose, all with a border of two rows of very narrow cells; the basal minute ovato-lanceolate, *the median ovate, elongato-lanceolate, with the margin involute and shortly* 3–4 *toothed at apex*, the uppermost narrowly lanceolate, scarcely toothed. Hyaline cells with annular fibres, upper with numerous small pores on each side of cell, lower lateral with large pores which become fewer in those at the middle of leaf. *Chlorophyll cells compressed, enclosed by the hyaline in the upper part of leaf, but in the lower part free both on the inner and outer surface, and rectangular in section.*

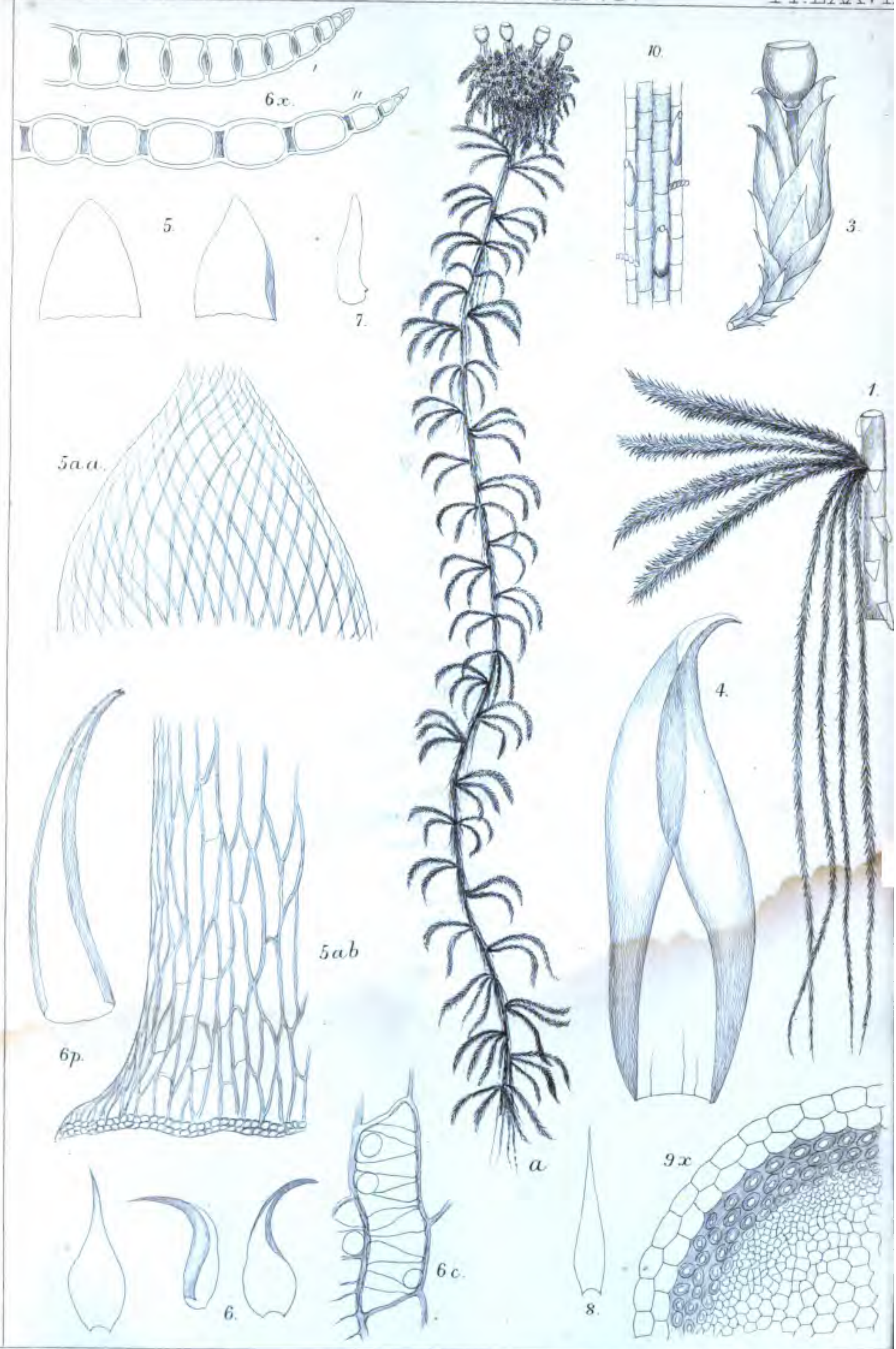
Male inflorescence purple, at the apex of the divergent branches.

Fruits clustered in the capitulum, but moderately exserted; perichætium straw-coloured or pink, lower bracts ovate acuminate, concave, recurved at apex, upper elongate oblong, slightly emarginate and somewhat recurved at point, convolute, without fibres or pores. Capsule small globose, blackish brown, spores pale yellow.

Var. *β. squarrosulum* Russow.

Leaves of the longer divergent branches squarrose, with more numerous pores.

Hab.—Wet pine-woods, rare. Techelfer woods near Dorpat, frequent (Girgensohn 1847); Kaddak near Reval, Allentacken



R. Braithwaite del. ad nat. W. West & Co. lith.

Sph. Wulfii.

and Appelsee (Russow); Jamni—Les near Permesküll (Gruner). Berglunda near Lycksele, Lapland (J. Ångstrom 1864). Belleville, Canada (Macoun, Fowler). Near New York (Howe, Peck, Austin). Fr. July.

This rare and beautiful species may be readily known by its clavate divergent branches and the large number of them in each fascicle, as well as by the small stem leaves and the dense globose capitulum; in all other points its affinity lies clearly with *Sph. acutifolium.*

EXPLANATION OF PLATE LXXVII.

Sphagnum Wulfii.

a.—Fertile plant.
1.—Part of stem and branch fascicle.
3.—Fruit and perichætium. 4.—Bract from same.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same. 5 *a b.*—Ditto of basal wing.
6.—Leaves from middle of a divergent branch. 6 *p.*—Point of same. 6 *x.*—Transverse sections, ′ from upper part, ″ from lower part. 6 *c.*—Cells ′ from upper part, ″ from lower lateral part × 200.
7.—Basal intermediate leaf.
8.—Leaf from a pendent branch.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

III.—*The Pebrine Corpuscles in the Silkworm, and what they are analogous to.*

In the year 1865, Pasteur was instructed by the French Minister of Agriculture to specially investigate and report upon the diseases incident to silkworms. During the interval between the years 1853 and 1865, these disorders had reduced the annual production of cocoons in France from sixty-five to ten millions of pounds. In the admirable work which resulted from his laborious researches,* the author remarks: "Certain disorders of the human race are accompanied by spots upon the skin, which originate in consequence of various alterations of the intestinal canal. This is not the sole observation applicable to human pathology which the experiments detailed in this work will suggest to the intelligent reader."

Diseases of the higher and lower orders of the animal kingdom are undoubtedly subject to similar conditions, in their genesis, resolution, or fatal issue. It is more logical as well as more consonant with scientific method, to observe the uniformity of a pathological law in the caries of an elephant's dentine, and the gangrene

* 'Étude sur la Maladie des Vers-à-soie, moyen pratique assuré de la combattre et d'en prévenir le retour,' par M. L. Pasteur, Membre de l'Institut Impérial de France, et de la Société Royale de Londres. Paris: 1870. This work is the source from which have been obtained all the facts relative to the contagious disease of the silkworm, to which reference is made in this paper.

of a spider's foot, than to seek with Huxley for a community of protoplasm between the finner whale and the fungus upon a fly. The ciliæ of the vorticella and of the human bronchi are not identical in structure, but they move in obedience to a similar impulse.

A medical friend once remarked to the writer of these pages, that the periodical visitations and ravages of insects presented a striking analogy to the recurrence and devastation of epidemic diseases. It is well worth the investigation to inquire if they be not alike dependent upon similar hygrometric and thermometric conditions of the soil and atmosphere.

It is proposed here to point out the analogies which exist between the pebrine of the silkworm and syphilis in man, not because these analogies might be so interpreted as to indicate that the two disorders have, in common, a parasitic origin. It is because the knowledge we at present possess relative to contagion is so scanty, that it may be said every new observation of its phenomena stimulates the belief that that which is unknown and yet knowable is largely in excess of that which is known regarding it.

Bumstead, referring to this subject in a recent paper,* says:

"The fact is, that a new field for investigation and experiment has been opened, which no one has as yet fully explored, and no one can pretend to understand. The exploration of this field promises to throw light, not only upon syphilis, but upon other contagious diseases, and even to add to our knowledge of the nature of specific poisons in general; but the work is yet undone, and any conclusions at this time are only premature."

It is preferable to select syphilis for the study of the analogies referred to above, first, because it is a disease produced by a tangible virus; and, second, because of the multiformity of its results. It is possible to secure upon the point of the lancet a drop of matter which we can prove to be capable of producing all the complications of the disease. This is also true of pebrine. While we have, however, an equal opportunity of isolating the *materies morbi* in vaccinia, variola, malignant pustule, and certain other maladies, the polymorphism of the results produced is not equally marked as a basis for comparison.

It is, perhaps, proper to admit, at the outset, that the investigations of Professors Stricker and Kobner have completely exploded the theories of Lostorfer, Salisbury, and others, as to the existence and causality of *crypta syphilitica.* We have no additional information which would warrant us in reviving such dead issues. That is not the purpose of this paper. It is here intended merely to exhibit a general agreement between the origin and evolution of two contagious diseases, existing in two widely-separated orders

* 'Am. Jour. of the Medical Sciences,' April, 1873.

of animals, in order that the classical feature of contagion in an extended area may be better appreciated.

The silkworm, as is well known, is the larve of the *Bombyx mori,* which deposits an ovum, from which, in turn, the caterpillar is produced. The latter, after undergoing four (in some races three) distinct changes of integument, becomes a pupa or chrysalis, and surrounds itself with the silk cocoon. From this, lastly, the perfect moth—imago of naturalists—effects its escape. When it is considered that, in a period of between thirty and thirty-five days, the caterpillar increases in size till it becomes eight or ten thousand times larger than the newly-escaped larve, it will be seen that organic life is displayed with unequalled activity in its development. Diseases, therefore, cannot but progress, *pari passu,* with an intensity proportioned to the energy of the vital forces.

In the human economy, of what paramount importance to its conservation are the critical phases of the first and second dentition, the arrival of puberty, and the change of the menopause! In the silkworm no less than seven equally important crises occur, during a comparatively short interval—the cycle of a brief existence, whose momentous stages offer unusual facilities for the encroachment of disease.

It is to be remarked, if we begin with the earliest phases of the two disorders, that,

1. *Pebrine and syphilis are alike producible by artificial inoculation.* Pasteur produced a liquid capable of inoculation, by bruising a diseased worm and mixing the mass with a small quantity of water. A number of worms were selected, carefully examined in order to ensure their soundness, and thoroughly cleansed by washing, so that no germs might remain in contact with the skin. He then made a small puncture in one of the posterior rings of the body of each, and inoculated the wound by inserting into it a needle dipped in the infecting liquid. The wounds readily cicatrized, and nothing but a black or dark-coloured spot was soon visible in the site of the puncture. Of twenty worms inoculated in this manner, on one occasion, seven became diseased to such an extent as to exhibit from fifty to two hundred of the corpuscles characteristic of pebrine, in one microscopic field. The experimenter explains why no larger proportion of successful inoculations was made: "The blood which escapes from the wound does not invariably permit of penetration by the corpuscles which are intended to produce infection." Audoin is said to have observed the same fact in his inoculations. Many an experienced physician has failed of successful vaccination for a similar reason.

It should be stated, however, that most frequently pebrine is produced by the ingestion of corpuscular germs when the worm is

feeding upon the mulberry leaf. The corpuscles are then found distributed over the surface of the leaf in débris; and a single repast is said to be sufficient to occasion the disease. It is worthy of note that an intestinal lesion is then produced.

It cannot be doubted that chancres would in like manner result, if, by any natural process, the secretion from similar sores could be applied to the mucous surface of the intestines. But it may well be doubted if this species of infection of the primæ viæ ever occurs in the human subject. A vacciniculturist of this city, however, once informed the writer that he was in receipt of numerous orders from practitioners of the homœopathic delusion, who desired to secure an infinitesimal quantity of vaccine virus, rubbed up with sugar of milk for internal administration!

2. *Pebrine and syphilis are alike communicable by accidental inoculation.* Pasteur discovered numerous cicatrices in healthy worms, which resulted from wounds. These wounds were inflicted by hooklets attached to the anterior organs of locomotion, in those caterpillars with which they had come into frequent contact. These were never seen in isolated individuals. He remarks that not infrequently these sharp hooklets, by which the caterpillar is enabled to cling to the leaf upon which it feeds, are inserted into the fæces or integument of diseased worms, and subsequently into the bodies of those that are sound, thus serving to propagate the disease by accidental inoculation. It is evident that there is here also the possibility of the production of *mediate* contamination, the porte-virus (if it be allowable to coin a suggestive word) being exempt from infection.

3. *Pebrine and syphilis alike require a period of incubation, before the phenomena of general disease appear.* Pasteur discovered that after accidental or artificial inoculation, and also after the ingestion of disease germs, a period of from ten to twelve days elapsed before external manifestations of pebrine appeared. By feeding a number of larves with the solution which has been already referred to, and by killing and carefully examining a fixed number of bodies at consecutive dates, he was enabled to follow the evolution of the disease, and to trace its natural history. In every instance the period of incubation was noted. This is such a constant concomitant of contagious diseases, that it may well be considered essential to their full development.*

4. *The first general indications of constitutional disease in pebrine and syphilis appear as integumentary lesions.* In the course of the experiments conducted by Pasteur, whenever a number of larves were selected for inoculation or infection, a similar number of the same age and habitat were set aside in a healthy condition, in order to serve the purposes of comparison. At the expiration of

* See 'Nouv. Dict. de Méd. et de Chi. Jaccoud,' art. "Contagion."

the period of incubation previously referred to, a very sensible inequality was noticeable in these two classes. Those which were left uninfected, displayed unmistakable evidence of greater well-being; while the diseased worms, when examined by the aid of a lens, exhibited numerous excessively small spots or maculæ, hitherto unnoticeable, about the head and rings. These lesions did not at first indicate the presence of the characteristic corpuscles in the skin. The "extension of the latter from centre to circumference had not yet affected the external organs. These surface spots," says Pasteur, "only occur when the internal skin, if I may be allowed the expression, is affected to such a degree as to seriously interfere with the functions of digestion and assimilation."

Subsequently, however, integumentary lesions were produced which, upon careful examination, were found to contain the pebrine corpuscles. It is difficult to recognize the distinction here established, and not recall the difference between those superficial syphilides, which disappear readily under appropriate treatment, and those which contain a specific morbid product. One instinctively recurs to the theory of Jonathan Hutchinson and others, that the lesions of secondary syphilis are febrile phenomena. These precede the deposits of tertiary forms, in which the "still-born" product of Lancereaux is to be distinguished.

The patches upon the integument in pebrine are generally of a dark colour, sometimes black (whence the name), some more and some less clearly defined. The petechial character of this stage of the disease has given it the name by which it is known among the Italians (Petechia of the Silkworms). When completely developed, these stains are surrounded by a yellowish areola, which exhibits various gradations in colour. Sometimes they constitute the sole symptoms of the disease.

M. Quatrefages, with whose opinions Pasteur is not in complete accord, declares that the alterations, described above, are best studied in the skin of the young larves. In these he could occasionally descry nothing more than a yellowish tint, slightly obscuring the hyaline transparency of the tissues. Somewhat later, a darker stain became visible, shading gradually into brown, until the translucence of the epidermis was lost. Finally, a brownish-black stigma remained, which was accompanied by a disappearance of all traces of organization. About this, as a nucleus, a yellowish areola extended, which, in his opinion, marked the invasion of the surrounding tissues. This process generally continued until arrested, either by the death of the worm, or by the regular replacement of the old by a new integument. In the course of two or three days, however, the new cuticle, which at first appeared entirely normal, was in its turn affected by the disease, "proving," says Quatrefages, "that the

lesions were not local phenomena, but signs of a constitutional malady, dependent upon a profound cause."

Pasteur has noted that the development of the pebrine corpuscles proceeds with an unexampled rapidity during these periods of metamorphosis—a circumstance which our knowledge of the laws of pathology would lead us to expect. He disagrees with Quatrefages in the supposition that the integumentary lesions are localized foci, from which a quasi-gangrenous process extends to the invasion of adjacent tissue; but considers each stigma to be a resultant of corpuscular development, and the changes in the appearance of the maculæ not due to molecular death, but to neoplastic hyperplasia.

In addition to the symptoms noted above, certain other indications of disease are described in the adult moth, as, for example, vesicles, varices, and bullæ filled with a sanguinolent fluid, under or near the wings. Some of these were observed to burst, and their contents, escaping and drying, were found to form adherent crusts, black and viscous, of the size of a pea.

5. *Pebrine and syphilis are alike productive of a specific adenopathy.* The secretion of the silk-glands of the pupa has solely contributed to the value placed upon the insect by the commercial world. In a pathological point of view, these glands possess especial importance from the fact that they are rapidly affected in pebrine. The large pentagonal cells which surround the canal where the silk is secreted in a viscous state, exhibit in a diseased condition numbers of oval corpuscles, crowded together, and sometimes collected in such masses that they lend an appearance of hypertrophy to the glandular tissue. Viewed with a low power, they exhibit whitish projections brilliant in colour, of oval form, and very clear definition. They are, without doubt, evidence of the extension of the disease to the visceral organs of the worms: and the total incapacity of the larves to produce cocoons—those of them, at least, which are profoundly affected—is a proof of the destructive agency exerted by the glandular neoplasms.

In syphilis, not only are those glands affected which are in the chain of the great system of lymphatics, but those which are actively concerned in hæmatopœsis. There is strong reason to believe that, aside from the development of hepatic gummata, usually found in the tertiary stage, one of the earliest symptoms of constitutional syphilis is dependent upon some disturbance of the glycogenic function of the liver. Dr. Charles Murchison has recently concluded,* after reviewing the discoveries of Hoppe Seyler, Bernard, Lehmann, McDonnell, Hirt, of Zittau, Weber, and Kölliker, that "the glycogen secreted in the liver cell combines with nitrogen and forms an azotised protoplasm which main-

* 'Lancet,' June, 1874.

tains the nutrition of the blood and tissues." In this light the chloroanæmia of early syphilis is most readily explained—a condition which is constant in all but benign cases, and which constitutes an important indication for successful treatment.

6. *Pebrine and syphilis are, alike, diseases of the blood.* In a healthy state the blood of the larve is a transparent albuminous fluid—colourless in the case of those races which produce white silk; and golden yellow in those which produce yellow silk. Under the microscope, innumerable spherical bodies appear, of various sizes, the largest of which does not in its greatest diameter exceed ·0039 of an inch. They seem endowed with individual vitality, and continually reproduce themselves during the life of the insect. When the latter is infected with pebrine, the number of the blood-globules decreases—thus inducing a species of chloroanæmia—and the albuminous fluid becomes charged with an immense number of minute animated corpuscles ·01 of an inch in diameter, increasing in proportion to the disappearance of its normal ingredients. These are the pebrine corpuscles already described, which Pasteur is disposed to regard as the parasitic germs of a species of psorosperm. They are oval or reniform in contour, destitute of ciliæ, and move rapidly, apparently at will, sometimes advancing and sometimes receding in the vascular channel.

The genus "psorosperm" was first established by Jean Muller, after his observation of certain anomalous organisms in different varieties of fishes, and especially in the fresh-water pike. But certain later expressions of Pasteur seem to imply that his mind is not perfectly clear as to the parasitic character of the germs described by him. In some of his communications to the Academy of Sciences, for example, he uses language from which it might be inferred that the disease originated in generations of the ancestors of these worms, whose connective tissue had undergone a peculiar cell-metamorphosis.

It is well known that Beale * adduces very strong grounds for the belief that contagious disease germs are not parasites, and his opinions are largely the result of researches upon the subject of the cattle-plague. Let it be supposed, in accordance with his views, that the corpuscles described by Pasteur are bioplasts—contagious living disease germs—that they are the descendants of blood or tissue bioplasts; that subsequently, either by hyper-nutrition or retrogression, they have undergone a conversion of energy, and become powerful to self-multiply indefinitely, and powerless to build up new and normal structures. This would explain the amœbiform movement of the pebrine corpuscles, their contagiousness, their virulence, and their destructiveness. Not only so, but it would do away with the need of resorting to a novel species of

* 'Disease Germs; their Nature and Origin.' Lionel S. Beale. London: 1872.

parasite, in order to explain the phenomena. It should be stated in this connection, that Beale considers the observations of both Pouchet and Pasteur open to objections upon the ground of their employment of very low powers. Many of the germs figured by Beale were viewed with an objective of one-fiftieth of an inch focal distance, enlarging the dimensions of these organisms 2800 diameters.

In such a field as this, speculation is illusory, and scientific deductions are alone to be desired. Still the general trend of the exposed strata is in one direction. They to whom the conservation and transmutation of forces is an unalterable fact of physics, have no difficulty in believing that there is a similar law to which the vital forces are subject. Heat, light, and electricity are shown to be modes of motion—interchangeable and intercurrent. The day is, perhaps, not far distant, when it will be clear that contagious and other diseases, which betray themselves by structural lesions, depend upon the mode of motion of the bioplast. This motion is known to be the measure of its energy. Can we not even declare that it is the essential condition of its vitality? Motionless bioplasm is dead. The transmutation of a normal to an abnormal energy should, therefore, produce disease and ultimate death. If this can be shown, it will be apparent that by an inversion of this process restoration from disease occurs.

Guerin-Meneville, in a report to the French Agricultural Society in 1849—mark the date!—gives expression to the same general thought. "It seems clear to me," said he, "that these granules (pebrine corpuscles) are the elements of new blood-globules, normally produced and launched into the vascular currents of healthy worms; but in pathological conditions they lack certain essential elements, and are therefore arrested in the progress of development."

Pasteur describes the mature corpuscles as brilliant of refraction and ovoid in shape. They subsequently become pyriform, surround themselves with a double envelope, and exhibit a slight flattening at the narrower extremity. They contain granules, either free or adherent to the cell-wall, and these, he believes, after their exit by rupture of the cell-envelope, serve as new centres for the development of new corpuscles, and thus extend the disease. The tissue of these organisms was supposed to contain sarcode.

7. *Pebrine and syphilis are hereditary disorders.* The transmission of the disease of the silkworm from one generation to another, has been the most fruitful source of evil in the propagation of the species. Unfortunately, before the microscope had been employed in the study of the malady, sericulturists could not be persuaded to believe that apparently healthy ova from parents

of equal apparent health, contained the seeds of the devastation which had blasted their hopes of profit for the preceding year. Such, however, has been too frequently the case; and the success of Pasteur in totally eliminating the disease from those nurseries in which his method was pursued, was due to his recognition of this fact. It is not a little remarkable in this connection, to observe that,

8. *In Pebrine, as in syphilis, when one parent only is affected with the disease, healthy offspring may be produced.* This general fact was demonstrated by a great number of experiments upon the coupling of moths, in which there was undoubted evidence of corpuscular disease either of the male or the female. It appeared also from these experiments, that ova entirely sound were generated occasionally by males who exhibited very extensive traces of the malady, when assorted with females who, while they were indubitably infected, yet exhibited very few of the pathognomonic lesions of pebrine. The experimenter explained these circumstances by the conditions incidental to the chrysalis. If the latter became infected with pebrine so as to exhibit corpuscles very soon after the formation of the cocoon, the moth and its ova were almost certain to be similarly diseased. But if this development did not occur until near the time for the escape of the imago, then the ova of the moth might be entirely sound. In the case of the syphilitic ovum, similar results are said to be declared, according as infection occurs early or late in utero-gestation.

Other analogies between these diseases obviously exist which might be in turn the subject of comment. Such, for example, are the involvement of the nervous system and centres in each—the infecundity of infected females who are liable to sterility and the production of blighted germs; the non-inoculability of the infectious matter obtained during the later stages of each disease, and the liability of each to complication by the advent of other maladies.

It should be stated that Pasteur himself is disposed to regard pebrine as analogous to pulmonary phthisis. But he is careful to announce that in establishing a resemblance between the facts which he has observed and those relative to diseases of the human race, he does not speak as an expert.* The hereditary influence of phthisis seems to have attracted his attention to this subject.

But there are many objections to this view founded upon the clinical history of tuberculosis. This latter disease is neither

* "Je désire toutefois que l'on sache bien que je parle en profane, lorsque j'établis des assimilations entre les faits que j'ai observés et les maladies humaines."

infectious, contagious, nor inoculable.* Nor does it produce a pathognomonic cutaneous lesion.

It is true, as stated by Pasteur, that children born of phthisical parents may, in some instances, merely become more or less sickly, while in others tubercle may be developed in different degrees at various ages. But one has not to consult the statistics of consumption in order to establish this diversity in the evolution of hereditary disease. Congenital syphilis may infect the ovum, the fœtus at term, and the infant newly born or which has survived for weeks and months. But this is not the limit of its effect. Massa narrates cases in which the disease was developed between three and eleven years of age; Balling, similar instances at the age of sixteen; Rosen, at eleven; Baumes, at four; Cazenave, at eighteen; Fournier, at twenty-five; Zambaco, at twenty-six.† Other authors cite cases which illustrate the same point. In the face of these observations who will venture to say, "Thus far doth it come, and no farther"?

In concluding the consideration of the general subject here discussed, few will refuse to concur with the opinions expressed by Dr. William Aitken. "The diseases of the lower animals," says this author, "rarely form any part of the study of the student of medicine. The diseases of plants are almost entirely neglected. Yet it is clear that until all these have been studied, and some steps taken to generalize the results, every conclusion in pathology regarding the nature of diseases must be the result of a limited experience from a limited field of observation."—*The Medical Examiner*, Chicago, July 15th, 1874.

IV.—*On the Microscopical Characters of the Sputum in Phthisis.*

By John Denis Macdonald, M.D., F.R.S., Staff-Surgeon R.N., Assistant Professor of Naval Hygiene, Netley Medical School.

Plates LXXVIII. and LXXIX.

It is a question whether, with all our progress in pathology, we can satisfactorily diagnose incipient phthisis by the microscopical characters of the sputum alone. Critically similar appearances are presented in the sputum of chronic hæmorrhagic catarrh, and in a very frequent sequel of ordinary pneumonia. Indeed there can be

* Bouillaud states that "the tuberculous virus is an hypothesis which up to the present time rests upon no exact nor trustworthy observation; and there does not exist a single instance of tuberculosis of the lungs, or of any part of the body, being produced in the human species by means of specific (virulent) inoculation." As to contagion, the experiments of Erdt, Villemin, Simon, Herard, and Clarke have been shown by Lebert, Nyss, Sanderson, and Fox, to demonstrate merely the irritative character of subcutaneous injections of putrid matter.

† Lancereaux, 'Traité de la Syphilis.' Paris: 1866.

but little doubt that under the designation of Phthisis, at least several distinct maladies are commonly included; and some of these even tend to the destruction of the lung tissue in a manner scarcely to be distinguished from the process of ulceration occurring in connection with softened tubercle. Nevertheless, the detection of this tissue in the expectoration is perhaps the clearest diagnostic mark of phthisis that the sputa can afford. Professor Bennett's case of the discovery of elastic lung tissue under the microscope, prior to the development of true phthisical symptoms, appears to have been followed by few, if any, other good examples of the kind. There is, however, great promise that with improved methods of examination, as, for instance, the process recommended and so successfully practised by Dr. Fenwick,* much may yet be done to elucidate this important point. It usually happens that at the time we are able to trace such anatomical elements in the sputa, the disease has sufficiently manifested its real nature by other attending signs and symptoms. It seems to be generally admitted that the characters of the elastic tissue of the air-cells are so distinctive, that there is little likelihood of anything else being mistaken for it, and it is quite true that an experienced microscopist may not confound other fibrous tissues with it. But the tyro must be made aware that many things of an extraneous nature will be sure to deceive him, unless he makes himself acquainted with the minute anatomy of the air-cells, more especially with the microscopical appearance and mode of distribution of their supporting fibrous tissue.

The accompanying Figs. 1 and 2, Plate LXXVIII., are selected for the guidance of those who may not be familiar with the structure of the air-cells. The first represents a portion of inflated and dried lung, showing the comparative size and general disposition of the air-cells, as seen with a half-inch power. The second was drawn from a small portion of recent lung divested of its epithelium, and treated with acetic acid to render the investing tissues more transparent, and thus to display the fibrous basis more distinctly (magnified about 300 diameters).

Should tubercular matter be deposited in the cells themselves, or between the layers of the basement membrane in contact with the fibrous tissue, the irritation thus induced will sooner or later lead to inflammatory action and the development of its products, lymph and pus. With the central softening and ultimate breaking down of the tubercle, an ulcerative process is set up, by which fragments of pulmonary tissue become detached and are expectorated with the surrounding pus and mucus from the bronchial membrane. Such fragments are therefore commonly to be found in the sputa of persons affected with phthisis, and the credit of having first discovered them by the aid of the microscope belongs to Professor

* See 'Lancet,' December 5th, 1868.

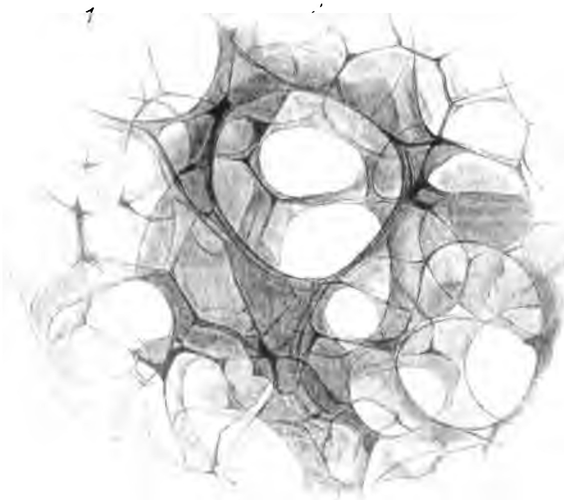
Schroeder Van der Kolk, who in 1846 first published his observations on the subject, and gave a fresh impetus to the labours of Dr. Andrew Clarke and others in this country. How soon after the accession of the malady, portions of lung tissue may be detected in the sputa, is yet a question to which no definite answer can be given, but at an advanced stage there is often very little difficulty in finding them. Thus Fig. 3 represents some fragments of fibrous tissue taken from a small quantity of greenish purulent mucus brought away with a single effort, but supposed by the patient, who had himself been a nurse, to be of some import, judging from his own sensations when it was expectorated. Patients frequently direct attention to particular portions of their sputa under a similar impression.

As to the various modes of discovering the presence of lung tissue in large or small masses of sputum, some remarks may be made here.

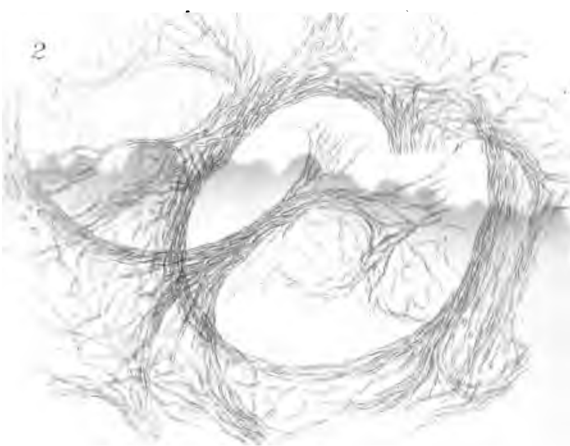
First of all, some difficulty may be experienced in removing suggestive morsels from the mass, on account of the viscid and ropy nature of the surrounding mucus, more or less imbued with pus. For this purpose Mr. Sansom has invented sharp spoon-blade forceps, which will be found most effective and useful; with this instrument small portions may be easily taken from different parts of the sputum, and separately examined. By careful compression the mucus and pus corpuscles, young and old epithelial cells, may be reduced to a thin film having a homogeneously granular appearance, in the midst of which any lung tissue present may be distinguished by its continuity and high refractive power as compared with that of the ground. The addition of a little acetic acid, however, will, on the principle previously explained, bring it out still more clearly.

When the quantity of expectoration is large it may not be easy to make a selection of suitable portions for examination, but by boiling the whole mass in caustic alkali the ropy plasticity of the mucus is reduced to a limpid fluidity, with the total destruction of the mucus and pus corpuscles. Any indestructible tissue present, such as cotton and linen fibre, the elastic tissue of beef or mutton used as food, or, what concerns us most, the fibrous tissue, &c., of the patient's lungs, will be deposited at the bottom of the vessel, from whence they may be readily removed with a pipette, and placed under the microscope. Besides the elastic tissue, which exhibits some slight change in its appearance by this treatment, we often find portions of the basement membrane of the air-cells and smaller bronchi, and even fragments of the bronchial radicles themselves, with unequivocal though faintly marked rings. (Fig. 3 *a*.) By pouring the sputum thus reduced into tall conical glasses such as are used for urinary deposits, all the solid matters will quickly settle at the bottom, so that some little discrimination may be required to distinguish true lung tissue from other substances with which it

1



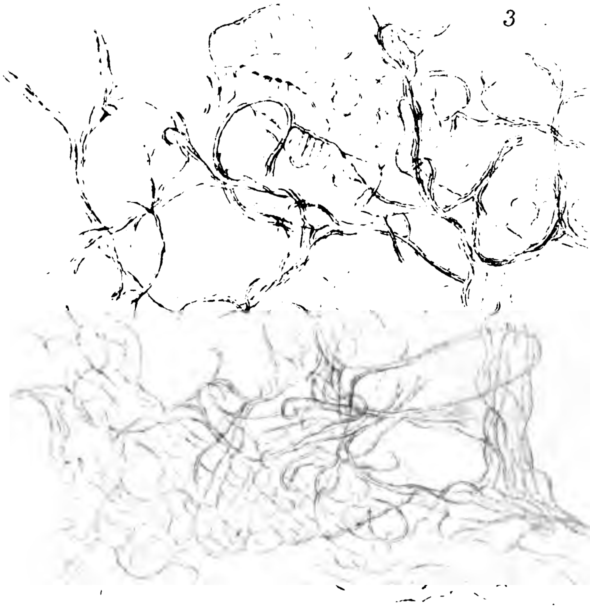
2



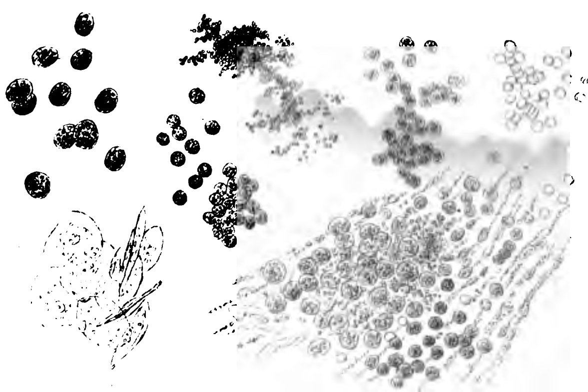
W. West & C^o. lith.

Sputum in Phthisis.

3



4



W. West & C^{o} lith.

Sputum in Phthisis.

is often entangled. Moreover, the sooner the deposit is examined the better, for by long standing, especially if there has been blood in the sputum, light cloudy films, which first occupy the upper part of the fluid, finally gravitate upon the tissue, and obscure it to a considerable extent when removed with a pipette and placed under the glass. The remedy for this, however, is shaking and reprecipitation. For the operation here alluded to, a small beaker adapted to the ring of a retort stand, a glass rod for stirring, a spirit lamp, a bottle with solution of soda, a tall champagne glass, and a pipette, are all the apparatus required.

When the case is complicated with much bronchitis, the first part of the expectoration with a fit of coughing usually consists of frothy mucus, after which it gradually becomes more dense and purulent, and better suited for examination. By receiving the sputum directly into the beaker at this period, much trouble may be saved, and there will be less chance of contracting impurities from without.

The more ordinary components of the sputa in phthisis are represented in Fig. 4, and are particularly noticed in the explanation of the Figures. — *Transactions of the St. Andrew's Medical Graduates' Association*, vol. vi.

EXPLANATION OF PLATES LXXVIII. AND LXXIX.

Plate LXXVIII.

Fig. 1.—A portion of lung, inflated and dried, in order to show the general arrangement and comparative size of the air-cells, as seen with a half-inch power.

" 2.—A portion of recent lung, divested of its epithelium and treated with acetic acid, so as to show the yellow elastic element and the nuclei of the white fibrous tissue more distinctly.

Plate LXXIX.

" 3.—(*a*) Lung tissue obtained from the sputum of a phthisical patient, by boiling in caustic soda, as described in the text. At * are one or two portions of minute bronchial tubes, with the rings faintly but unequivocally visible.

(*b*) Another specimen from the same sputum, with a strip of bronchial basement membrane and subjacent elastic tissue.

" 4.—The more ordinary components of phthisical sputa:—

(*a*) So-called exudation cells, filled with fatty granules, and occasionally speckled with pigment.

(*b*) The liberated contents of (*a*), the globules in some instances running together.

(*c*) Mucus-corpuscles.

(*d*) Pus-corpuscles.

(*e*) Blood-corpuscles.

(*f*) Epithelial scales.

(*g*) All the foregoing materials, with a basis of pure glairy mucus, exhibiting a striated appearance; and an alteration in the figure of some of the corpuscles by pressure and traction, showing the plastic nature of their contents. The mucus, pus, exudation, and epithelial cells are but modifications of the same essential organism.

V.—*Blue and Violet Stainings for Vegetable Tissues.*

By Christopher Johnston, M.D., Baltimore, U.S.A.

Without questioning the advantage of viewing vegetable tissues, isolated or in section, in glycerine or water when freshly obtained, I proceed at once to point out methods by which very useful and beautiful tinted objects of this class may be prepared and mounted in balsam.

As in the Bessemer process, iron must be thoroughly decarbonized and afterwards regularly dosed with carbon for the making of steel, so all colour must be destroyed in or withdrawn from vegetable structures before a good staining material can be successfully employed. Alcohol decolorizes, but its prolonged action crisps the tender material; and although the latter might take the dye, it would not always be possible to display the vegetable web, or, in many instances, to render it distinctly transparent. But alcohol is extremely important, indeed, I should rather say indispensable, in the preparation of the class of objects under consideration. For whatever may have been the early steps of several processes, alcoholic saturation must precede balsamic embalming.

With our present intention recent plant structures may be studied in two conditions, first, in that requiring the section-cutter, and, secondly, in their natural form. And this, without taking into account the ultimate dissection accomplished by the knife, by needles, by acids, or by alkalies. As a preliminary to the first it is only necessary to keep fresh specimens of plants or parts of plants in strong alcohol, in which, after the lapse of a month or two, colour will oftentimes entirely disappear. Whereupon the desired sections may be made, tinted if they be sufficiently blanched, and mounted at once.

For tinting such sections I have used the lilac fluid of Thiersch as given in Frey, a double strength lilac fluid, a dilution of the hæmatoxylin staining fluid of Dr. J. W. S. Arnold of New York, as published in the 'Lens' of July 1872, and aqueous solution of aniline blue which I obtained ready made and labelled Blue ink of F. G. Bower and Co., New York. I much prefer the two latter because distinct carmine stainings fatigue the eye when studied at night, while the lilac of logwood, but especially the delicate blue purple of the aniline, are most agreeable and not fatiguing at all.

Arnold's fluid is prepared as follows :—"The ordinary logwood extract is finely pulverized in a mortar, and about three times its bulk of alum (in powder) added ; the two ingredients are well rubbed up together, and mixed with a small quantity of distilled water. The complete admixture of the alum and hæmatoxylin is necessary, and this will require fifteen or twenty minutes' vigorous trituration.

More water may now be poured on, and the solution, after filtration, should present a clear, somewhat dark violet colour. If a dirty red be obtained more alum must be incorporated and the mixture again filtered. After standing several days add 75 per cent. alcohol in the proportion of two drachms to one ounce of the fluid. Should a scum form on the surface of the liquid at any time, a few drops of alcohol and careful filtering will be all that is required.

This fluid colours very rapidly, requiring but a few minutes, whereas, if a slower tinting be desired, the fluid may be diluted with a mixture of one part alcohol and three parts water.*

Aniline blue, of which there are several shades, may be dissolved in distilled water. A 1 per cent. solution, to which a small quantity of alcohol and a trace of oxalic acid have been added, answers admirably. For convenience I use "Bower's Blue Ink," slightly acidulated with oxalic acid. The tint is exactly that of Robert Dale and Co.'s Soluble Blue, No. 3.

The hæmatoxylin colour will not wash out, but the aniline blue will do so unless precaution be taken.

Having prepared suitable sections of parts having been kept in alcohol, place them in a very weak dilution of Arnold's fluid, and watching the result, transfer the morsel to dilute alcohol for washing, and afterwards to strong alcohol in anticipation of mounting.

Or immerse the sections in the aniline fluid for five or ten minutes, or longer; watch the result; wash in strong alcohol, and drop them into absolute alcohol.

The logwood stainings may be mounted at pleasure; but the aniline dyeings ought to be rendered transparent in oil colours as soon as possible, and mounted speedily in a moderately thin solution of old hard balsam. If it be intended to display the general structure let the tint be decided; but if it be wished to give prominence to the vessels, for instance, a faint blue only should suggest the other parts.

A weight of a fourth or half-ounce ought to be placed on the cover for a week.

The treatment of thin leaves, or of fresh green sections, is entirely different. Colour must first be removed, or else staining would be of little service. The bleaching is to be accomplished through the agency of Labarraque's solution of chlorinated soda, in which the objects ought to be macerated, and suffered to remain until perfectly achromatic and transparent. Immediately thereafter others must be transferred to distilled water for an hour or two, and then to a 3 per cent. solution of oxalic acid in 50 per cent. alcohol, which neutralizes the soda and disposes the tissue to accept the *aniline* dye.

* Soluble Blue, No. 3, Robert Dale and Co., Hulme, Manchester, England, Soluble Blue, Simpson and Maule, London.

At the operator's pleasure the chlorinated leaves may be soaked in pure water for an hour, and afterwards in a 3 per cent. aqueous solution of alum, in preparation for the *logwood* staining.

If the *aniline* blue dye be chosen, let the acidulated leaves be immersed in the blue fluid, and soaked for twenty or thirty minutes. Upon being withdrawn their colour will be found to be very intense, but washing in 90 per cent. alcohol for half a minute will remove superfluous aniline, and a final bath in absolute alcohol will in a few minutes prepare the object for being soaked in oil of cloves. With a bent platinum spatula the transparent preparations must be laid upon a slide, receive a liberal quantity of a solution of old balsam in chloroform poured upon it, and be covered with thin glass, on which a small weight is to be placed.

In the course of a month or two the excess of balsam may be cleaned off, but the slide should bear a provisional label before the specimen is mounted.

The hæmatoxylin staining is, perhaps, rather more successful than the aniline, and its violet tinge is very beautiful indeed. Let the transparent leaves or sections be transferred from the alum solution, after a short residence, to Arnold's fluid undiluted. At the end of five minutes remove one object and wash it in the alum solution; if not decidedly or sufficiently tinted return to the fluid and examine it again at the end of another five minutes; and when, finally, the whole of the object shall have been evenly and distinctly coloured, it must be dropped into the alum solution for a minute or two, and then into 75 per cent. alcohol, in which the former dull red colour will change to a lovely violet. In many cases the fluid, diluted with 25 per cent. alcohol, will make a better staining, but, of course, the object must have a longer exposure.

For mounting immerse in absolute alcohol, then in oil of cloves, and finally embalm in the chloriform solution of old Canada balsam.

If the foregoing directions be strictly followed there will be no difficulty with the logwood stainings; but to secure good aniline preparations all the steps of the process must be done completely but with all possible dispatch, therein lies a secret of success.

It is, perhaps, somewhat out of place in this paper to speak of stainings of animal tissues; but I beg to say that my results have not, hitherto, been very satisfactory as regards aniline colours; while, on the contrary, carmine has always succeeded perfectly, and Arnold's fluid most beautifully, with sections of the nervous centres.

VI.—*A Physicist on Evolution:* being a part of PROFESSOR TYNDALL's Address to the British Association at Belfast.

BISHOP BUTLER accepted with unwavering trust the chronology of the Old Testament, describing it as "confirmed by the natural and civil history of the world, collected from common historians, from the state of the earth, and from the late inventions of arts and sciences." These words mark progress: they must seem somewhat hoary to the Bishop's successors of to-day.* It is hardly necessary to inform you that since his time the domain of the naturalist has been immensely extended—the whole science of geology, with its astounding revelations regarding the life of the ancient earth, having been created. The rigidity of old conceptions has been relaxed, the public mind being rendered gradually tolerant of the idea that not for six thousand, nor for sixty thousand, nor for six thousand thousand, but for æons embracing untold millions of years, this earth has been the theatre of life and death. The riddle of the rocks has been read by the geologist and palæontologist, from sub-cambrian depths to the deposits thickening over the sea-bottoms of to-day. And upon the leaves of that stone book are, as you know, stamped the characters, plainer and surer than those formed by the ink of history, which carry the mind back into abysses of past time, compared with which the periods which satisfied Bishop Butler cease to have a visual angle. Everybody now knows this; all men admit it; still, when they were first broached, these verities of science found loud-tongued denunciators, who proclaimed, not only their baselessness considered scientifically, but their immorality considered as questions of ethics and religion: the Book of Genesis had stated the question in a different fashion; and science must necessarily go to pieces when it clashed with this authority. And as the seed of the thistle produces a thistle, and nothing else, so these objectors scatter their germs abroad, and reproduce their kind, ready to play again the part of their intellectual progenitors, to show the same virulence, the same ignorance, to achieve for a time the same success, and finally to suffer the same inexorable defeat. Sure the time must come at last when human nature in its entirety, whose legitimate demands it is admitted science alone cannot satisfy, will find interpreters and expositors of a different stamp from those rash and ill-informed persons who have been hitherto so ready to hurl themselves against every new scientific

* Only to some; for there are dignitaries who even now speak of the earth's rocky crust as so much building material prepared for man at the Creation. Surely it is time that this loose language should cease.

revelation, lest it should endanger what they are pleased to consider theirs.

The lode of discovery once struck, those petrified forms in which life was at one time active, increased to multitudes and demanded classification. The general fact soon became evident that none but the simplest forms of life lie lowest down, that as we climb higher and higher among the superimposed strata more perfect forms appear. The change, however, from form to form was not continuous—but by steps, some small, some great. "A section," says Mr. Huxley, "a hundred feet thick will exhibit at different heights a dozen species of ammonite, none of which passes beyond its particular zone of limestone, or clay, into the zone below it or into that above it." In the presence of such facts it was not possible to avoid the question, Have these forms, showing, though in broken stages and with many irregularities, this unmistakable general advance, been subjected to no continuous law of growth or variation? Had our education been purely scientific, or had it been sufficiently detached from influences which, however ennobling in another domain, have always proved hindrances and delusions when introduced as factors into the domain of physics, the scientific mind never could have swerved from the search for a law of growth, or allowed itself to accept the anthropomorphism which regarded each successive stratum as a kind of mechanic's bench for the manufacture of the new species out of all relation to the old.

Biassed, however, by their previous education, the great majority of naturalists invoked a special creative act to account for the appearance of each new group of organisms. Doubtless there were numbers who were clear-headed enough to see that this was no explanation at all, that in point of fact it was an attempt, by the introduction of a greater difficulty, to account for a less. But having nothing to offer in the way of explanation, they for the most part held their peace. Still the thoughts of reflecting men naturally and necessarily simmered round the question. De Maillet, a contemporary of Newton, has been brought into notice by Prof. Huxley as one who "had a notion of the modifiability of living forms." In my frequent conversations with him, the late Sir Benjamin Brodie, a man of highly philosophical mind, often drew my attention to the fact that, as early as 1794, Charles Darwin's grandfather was the pioneer of Charles Darwin. In 1801, and in subsequent years, the celebrated Lamarck, who produced so profound an impression on the public mind through the vigorous exposition of his views by the author of 'Vestiges of Creation,' endeavoured to show the development of species out of changes of habit and external condition. In 1813, Dr. Wells, the founder of our present theory of dew, read before the Royal Society a paper in

which, to use the words of Mr. Darwin, "he distinctly recognizes the principle of natural selection; and this is the first recognition that has been indicated." The thoroughness and skill with which Wells pursued his work, and the obvious independence of his character, rendered him long ago a favourite with me; and it gave me the liveliest pleasure to alight upon this additional testimony to his penetration. Prof. Grant, Mr. Patrick Matthew, Von Buch, the author of the 'Vestiges,' D'Halloy, and others,* by the enunciation of views more or less clear and correct, showed that the question had been fermenting long prior to the year 1858, when Mr. Darwin and Mr. Wallace simultaneously but independently placed their closely concurrent views upon the subject before the Linnean Society.

These papers were followed in 1859 by the publication of the first edition of 'The Origin of Species.' All great things come slowly to the birth. Copernicus, as I informed you, pondered his great work for thirty-three years. Newton for nearly twenty years kept the idea of Gravitation before his mind; for twenty years also he dwelt upon his discovery of Fluxions, and doubtless would have continued to make it the object of his private thought had he not found that Leibnitz was upon his track. Darwin for two-and-twenty years pondered the problem of the origin of species, and doubtless he would have continued to do so had he not found Wallace upon his track.† A concentrated but full and powerful epitome of his labours was the consequence. The book was by no means an easy one; and probably not one in every score of those who then attacked it had read its pages through, or were competent to grasp their significance if they had. I do not say this merely to discredit them; for there were in those days some really eminent scientific men, entirely raised above the heat of popular prejudice, willing to accept any conclusion that science had to offer, provided it was duly backed by fact and argument, and who entirely mistook Mr. Darwin's views. In fact the work needed an expounder; and it found one in Mr. Huxley. I know nothing more admirable in the way of scientific exposition than those early articles of his on the origin of species. He swept the curve of discussion through the really significant points of the subject, enriched his exposition with profound original remarks and reflections, often summing up in a single pithy sentence an argument which a less compact mind would have spread over pages. But there is one impression made by the book itself which no exposition of it, however luminous, can

* In 1855 Mr. Herbert Spencer ('Principles of Psychology,' 2nd edit., vol. i., p. 465) expressed "the belief that life under all its forms has arisen by an unbroken evolution, and through the instrumentality of what are called natural causes.

† The behaviour of Mr. Wallace in relation to this subject has been dignified in the highest degree.

convey; and that is the impression of the vast amount of labour, both of observation and of thought, implied in its production. Let us glance at its principles.

It is conceded on all hands that what are called varieties are continually produced. The rule is probably without exception. No chick and no child is in all respects and particulars the counterpart of its brother or sister; and in such differences we have "variety" incipient. No naturalist could tell how far this variation could be carried; but the great mass of them held that never by any amount of internal or external change, nor by the mixture of both, could the offspring of the same progenitor so far deviate from each other as to constitute different species. The function of the experimental philosopher is to combine the conditions of nature and to produce her results; and this was the method of Darwin.* He made himself acquainted with what could, without any matter of doubt, be done in the way of producing variation. He associated himself with pigeon-fanciers—bought, begged, kept, and observed every breed that he could obtain. Though derived from a common stock, the diversities of these pigeons were such that "a score of them might be chosen which, if shown to an ornithologist, and he were told that they were wild birds, would certainly be ranked by him as well-defined species." The simple principle which guides the pigeon-fancier, as it does the cattle-breeder, is the selection of some variety that strikes his fancy, and the propagation of this variety by inheritance. With his eye still upon the particular appearance which he wishes to exaggerate, he selects it as it reappears in successive broods, and thus adds increment to increment until an astonishing amount of divergence from the parent type is effected. Man in this case does not produce the *elements* of the variation. He simply observes them, and by selection adds them together until the required result has been obtained. "No man," says Mr. Darwin, "would ever try to make a fantail till he saw a pigeon with a tail developed in some slight degree in an unusual manner, or a pouter until he saw a pigeon with a crop of unusual size." Thus nature gives the hint, man acts upon it, and by the law of inheritance exaggerates the deviation.

Having thus satisfied himself by indubitable facts that the organization of an animal or of a plant (for precisely the same treatment applies to plants) is to some extent plastic, he passes from variation under domestication to variation under nature. Hitherto we have dealt with the adding together of small changes by the conscious selection of man. Can Nature thus select? Mr. Darwin's answer is, "Assuredly she can." The number of

* The first step only towards experimental demonstration has been taken. Experiments now begun might, a couple of centuries hence, furnish data of incalculable value, which ought to be supplied to the science of the future.

living things produced is far in excess of the number that can be supported; hence at some period or other of their lives there must be a struggle for existence; and what is the infallible result? If one organism were a perfect copy of the other in regard to strength, skill, and agility, external conditions would decide. But this is not the case. Here we have the fact of variety offering itself to nature, as in the former instance it offered itself to man; and those varieties which are least competent to cope with surrounding conditions will infallibly give way to those that are competent. To use a familiar proverb, the weakest comes to the wall. But the triumphant fraction again breeds to over-production, transmitting the qualities which secured its maintenance, but transmitting them in different degrees. The struggle for food again supervenes, and those to whom the favourable quality has been transmitted in excess will assuredly triumph. It is easy to see that we have here the addition of increments favourable to the individual still more rigorously carried out than in the case of domestication; for not only are unfavourable specimens not selected by nature, but they are destroyed. This is what Mr. Darwin calls "natural selection," which "acts by the preservation and accumulation of small inherited modifications, each profitable to the preserved being." With this idea he interpenetrates and leavens the vast store of facts that he and others have collected. We cannot, without shutting our eyes through fear or prejudice, fail to see that Darwin is here dealing, not with imaginary, but with true causes; nor can we fail to discern what vast modifications may be produced by natural selection in periods sufficiently long. Each individual increment may resemble what mathematicians call a "differential" (a quantity indefinitely small); but definite and great changes may obviously be produced by the integration of these infinitesimal quantities through practically infinite time.

If Darwin, like Bruno, rejects the notion of creative power acting after human fashion, it certainly is not because he is unacquainted with the numberless exquisite adaptations on which this notion of a supernatural artificer was founded. His book is a repository of the most startling facts of this description. Take the marvellous observation which he cites from Dr. Crüger, where a bucket with an aperture, serving as a spout, is formed in an orchid. Bees visit the flower: in eager search of material for their combs they push each other into the bucket, the drenched ones escaping from their involuntary bath by the spout. Here they rub their backs against the viscid stigma of the flower and obtain glue; then against the pollen-masses, which are thus stuck to the back of the bee and carried away. "When the bee, thus provided, flies to another flower, or to the same flower a second time, and is pushed by its comrades into the bucket, and then crawls out by the passage, the

pollen-mass upon its back necessarily comes first into contact with the viscid stigma," which takes up the pollen; and this is how that orchid is fertilized. Or take this other case of the *Catasetum*. "Bees visit these flowers in order to gnaw the labellum; on doing this they inevitably touch a long, tapering, sensitive projection. This, when touched, transmits a sensation or vibration to a certain membrane, which is instantly ruptured, setting free a spring by which the pollen-mass is shot forth like an arrow in the right direction, and adheres by its viscid extremity to the back of the bee." In this way the fertilizing pollen is spread abroad.

It is the mind thus stored with the choicest materials of the teleologist that rejects teleology, seeking to refer these wonders to natural causes. They illustrate, according to him, the method of nature, not the "technic" of a man-like artificer. The beauty of flowers is due to natural selection. Those that distinguish themselves by vividly contrasting colours from the surrounding green leaves are most readily seen, most frequently visited by insects, most often fertilized, and hence most favoured by natural selection. Coloured berries also readily attract the attention of birds and beasts, which feed upon them, spread their manured seeds abroad, thus giving trees and shrubs possessing such berries a greater chance in the struggle for existence.

With profound analytic and synthetic skill, Mr. Darwin investigates the cell-making instinct of the hive-bee. His method of dealing with it is representative. He falls back from the more perfectly to the less perfectly developed instinct—from the hive-bee to the humble-bee, which uses its own cocoon as a comb, and to classes of bees of intermediate skill, endeavouring to show how the passage might be gradually made from the lowest to the highest. The saving of wax is the most important point in the economy of bees. Twelve to fifteen pounds of dry sugar are said to be needed for the secretion of a single pound of wax. The quantities of nectar necessary for the wax must therefore be vast; and every improvement of constructive instinct which results in the saving of wax is a direct profit to the insect's life. The time that would otherwise be devoted to the making of wax is now devoted to the gathering and storing of honey for winter food. He passes from the humble-bee with its rude cells, through the Melipona with its more artistic cells, to the hive-bee with its astonishing architecture. The bees place themselves at equal distances apart upon the wax, sweep and excavate equal spheres round the selected points. The spheres intersect, and the planes of intersection are built up with thin laminæ. Hexagonal cells are thus formed. This mode of treating such questions is, as I have said, representative. He habitually retires from the more perfect and complex, to the less perfect and simple, and carries you with him through stages of *perfecting*, adds

increment to increment of infinitesimal change, and in this way gradually breaks down your reluctance to admit that the exquisite climax of the whole could be a result of natural selection.

Mr. Darwin shirks no difficulty; and, saturated as the subject was with his own thought, he must have known, better than his critics, the weakness as well as the strength of his theory. This of course would be of little avail were his object a temporary dialectic victory instead of the establishment of a truth which he means to be everlasting. But he takes no pains to disguise the weakness he has discerned; nay, he takes every pains to bring it into the strongest light. His vast resources enable him to cope with objections started by himself and others, so as to leave the final impression upon the reader's mind that if they be not completely answered they certainly are not fatal. Their negative force being thus destroyed, you are free to be influenced by the vast positive mass of evidence he is able to bring before you. This largeness of knowledge and readiness of resource render Mr. Darwin the most terrible of antagonists. Accomplished naturalists have levelled heavy and sustained criticisms against him—not always with the view of fairly weighing his theory, but with the express intention of exposing its weak points only. This does not irritate him. He treats every objection with a soberness and thoroughness which even Bishop Butler might be proud to imitate, surrounding each fact with its appropriate detail, placing it in its proper relations, and usually giving it a significance which, as long as it was kept isolated, failed to appear. This is done without a trace of ill-temper. He moves over the subject with the passionless strength of a glacier; and the grinding of the rocks is not always without a counterpart in the logical pulverization of the objector. But though in handling this mighty theme all passion has been stilled, there is an emotion of the intellect incident to the discernment of new truth which often colours and warms the pages of Mr. Darwin. His success has been great; and this implies not only the solidity of his work, but the preparedness of the public mind for such a revelation. On this head a remark of Agassiz impressed me more than anything else. Sprung from a race of theologians, this celebrated man combated to the last the theory of natural selection. One of the many times I had the pleasure of meeting him in the United States was at Mr. Winthrop's beautiful residence at Brookline, near Boston. Rising from luncheon, we all halted as if by a common impulse in front of a window, and continued there a discussion which had been started at table. The maple was in its autumn glory; and the exquisite beauty of the scene outside seemed, in my case, to interpenetrate without disturbance the intellectual action. Earnestly, almost sadly, Agassiz turned and said to the gentlemen standing round, "I confess that

I was not prepared to see this theory received as it has been by the best intellects of our time. Its success is greater than I could have thought possible."

In our day great generalizations have been reached. The theory of the origin of species is but one of them. Another, of still wider grasp and more radical significance, is the doctrine of the Conservation of Energy, the ultimate philosophical issues of which are as yet but dimly seen—that doctrine which "binds nature fast in fate" to an extent not hitherto recognized, exacting from every antecedent its equivalent consequent, from every consequent its equivalent antecedent, and bringing vital as well as physical phenomena under the dominion of that law of causal connection which, as far as the human understanding has yet pierced, asserts itself everywhere in nature. Long in advance of all definite experiment upon the subject, the constancy and indestructibility of matter had been affirmed; and all subsequent experience justified the affirmation. Later researches extended the attribute of indestructibility to force. This idea, applied in the first instance to inorganic, rapidly embraced organic nature. The vegetable world, though drawing almost all its nutriment from invisible sources, was proved incompetent to generate anew either matter or force. Its matter is for the most part transmuted air; its force transformed solar force. The animal world was proved to be equally uncreative, all its motive energies being referred to the combustion of its food. The activity of each animal as a whole was proved to be the transferred activities of its molecules. The muscles were shown to be stores of mechanical force, potential until unlocked by the nerves, and then resulting in muscular contractions. The speed at which messages fly to and fro along the nerves was determined, and found to be, not as had been previously supposed, equal to that of light or electricity, but less than the speed of a flying eagle.

This was the work of the physicist: then came the conquests of the comparative anatomist and physiologist, revealing the structure of every animal, and the function of every organ in the whole biological series, from the lowest zoophyte up to man. The nervous system had been made the object of profound and continued study, the wonderful and, at bottom, entirely mysterious controlling power which it exercises over the whole organism, physical and mental, being recognized more and more. Thought could not be kept back from a subject so profoundly suggestive. Besides the physical life dealt with by Mr. Darwin, there is a psychical life presenting similar gradations, and asking equally for a solution. How are the different grades and orders of mind to be accounted for? What is the principle of growth of that mysterious power which on our planet culminates in Reason? These are questions which, though not thrusting themselves so forcibly upon the atten-

tion of the general public, had not only occupied many reflecting minds, but had been formally broached by one of them before the 'Origin of Species' appeared.

The *origination* of life is a point lightly touched upon, if at all, by Mr. Darwin and Mr. Spencer. Diminishing gradually the number of progenitors, Mr. Darwin comes at length to one "primordial form"; but he does not say, as far as I remember, how he supposes this form to have been introduced. He quotes with satisfaction the words of a celebrated author and divine who had "gradually learnt to see that it is just as noble a conception of the Deity to believe He created a few original forms, capable of self-development into other and needful forms, as to believe that He required a fresh act of creation to supply the voids caused by the action of His laws." What Mr. Darwin thinks of this view of the introduction of life I do not know. Whether he does or does not introduce his "primordial form" by a creative act, I do not know. But the question will inevitably be asked, "How came the form there?" With regard to the diminution of the number of created forms, one does not see that much advantage is gained by it. The anthropomorphism, which it seemed the object of Mr. Darwin to set aside, is as firmly associated with the creation of a few forms as with the creation of a multitude. We need clearness and thoroughness here. Two courses, and two only, are possible. Either let us open our doors freely to the conception of creative acts, or abandoning them, let us radically change our notions of matter. If we look at matter as pictured by Democritus, and as defined for generations in our scientific text-books, the absolute impossibility of any form of life coming out of it would be sufficient to render any other hypothesis preferable; but the definitions of matter given in our text-books were intended to cover its purely physical and mechanical properties. And taught as we have been to regard these definitions as complete, we naturally and rightly reject the monstrous notion that out of *such* matter any form of life could possibly arise. But are the definitions complete? Everything depends on the answer to be given to this question. Trace the line of life backwards, and see it approaching more and more to what we call the purely physical condition. We reach at length those organisms which I have compared to drops of oil suspended in a mixture of alcohol and water. We reach the *protogenes* of Haeckel, in which we have "a type distinguishable from a fragment of albumen only by its finely granular character." Can we pause here? We break a magnet and find two poles in each of its fragments. We continue the process of breaking, but however small the parts, each carries with it, though enfeebled, the polarity of the whole. And when we can break no longer, we prolong the intellectual vision to the polar molecules. Are we not urged to do *something* similar in the case

of life? Is there not a temptation to close to some extent with Lucretius, when he affirms that "Nature is seen to do all things spontaneously of herself without the meddling of the gods"? or with Bruno, when he declares that matter is not "that mere empty *capacity* which philosophers have pictured her to be, but the universal mother who brings forth all things as the fruit of her own womb"? The questions here raised are inevitable. They are approaching us with accelerated speed, and it is not a matter of indifference whether they are introduced with reverence or irreverence. Abandoning all disguise, the confession that I feel bound to make before you is that I prolong the vision backward across the boundary of the experimental evidence, and discern in that matter, which we in our ignorance, and notwithstanding our professed reverence for its Creator, have hitherto covered with opprobrium, the promise and potency of every form and quality of life.

The "materialism" here enunciated may be different from what you suppose, and I therefore crave your gracious patience to the end. "The question of an external world," says Mr. J. S. Mill, "is the great battle-ground of metaphysics."* Mr. Mill himself reduces external phenomena to "possibilities of sensation." Kant, as we have seen, made time and space "forms" of our own intuitions. Fichte, having first by the inexorable logic of his understanding proved himself to be a mere link in that chain of eternal causation which holds so rigidly in nature, violently broke the chain by making nature, and all that it inherits, an apparition of his own mind.† And it is by no means easy to combat such notions. For when I say I see you, and that I have not the least doubt about it, the reply is, that what I am really conscious of is an affection of my own retina. And if I urge that I can check my sight of you by touching you, the retort would be that I am equally transgressing the limits of fact; for what I am really conscious of is, not that you are there, but that the nerves of my hand have undergone a change. All we hear, and see, and touch, and taste, and smell, are, it would be urged, mere variations of our own condition, beyond which, even to the extent of a hair's breadth, we cannot go. That anything answering to our impressions exists outside of ourselves is not a *fact*, but an *inference*, to which all validity would be denied by an idealist like Berkeley, or by a sceptic like Hume. Mr. Spencer takes another line. With him, as with the uneducated man, there is no doubt or question as to the existence of an external world. But he differs from the uneducated, who think that the world really *is* what consciousness represents it to be. Our states of consciousness are mere *symbols* of an outside entity which produces them and determines the order of their succession, but the real nature of

* 'Examination of Hamilton,' p. 154. † 'Bestimmung des Menschen.'

which we can never know.* In fact the whole process of evolution is the manifestation of a Power absolutely inscrutable to the intellect of man. As little in our day as in the days of Job can man by searching find this Power out. Considered fundamentally, it is by the operation of an insoluble mystery that life is evolved, species differentiated, and mind unfolded from their prepotent elements in the immeasurable past. There is, you will observe, no very rank materialism here.

The strength of the doctrine of evolution consists, not in an experimental demonstration (for the subject is hardly accessible to this mode of proof), but in its general harmony with the method of nature as hitherto known. From contrast, moreover, it derives enormous relative strength. On the one side we have a theory (if it could with any propriety be so called) derived, as were the theories referred to at the beginning of this address, not from the study of nature, but from the observation of men—a theory which converts the Power whose garment is seen in the visible universe into an Artificer, fashioned after the human model, and acting by broken efforts as man is seen to act. On the other side we have the conception that all we see around us, and all we feel within us—the phenomena of physical nature as well as those of the human mind—have their unsearchable roots in a cosmical life, if I dare apply the term, an infinitesimal span of which only is offered to the investigation of man. And even this span is only knowable in part. We can trace the development of a nervous system, and correlate with it the parallel phenomena of sensation and thought. We see with undoubting certainty that they go hand in hand. But we try to soar in a vacuum the moment we seek to comprehend the connection between them. An Archimedean fulcrum is here required which the human mind cannot command; and the effort to solve the problem, to borrow an illustration from an illustrious friend of mine, is like the effort of a man trying to lift himself by his own waistband. All that has been here said is to be taken in connection with this fundamental truth. When "nascent senses" are spoken of, when "the differentiation of a tissue at first vaguely sensitive all

* In a paper, at once popular and profound, entitled "Recent Progress in the Theory of Vision," contained in the volume of letters by Helmholtz, published by Longmans, this symbolism of our states of consciousness is also dwelt upon. The impressions of sense are the mere *signs* of external things. In this paper Helmholtz contends strongly against the view that the consciousness of space is inborn; and he evidently doubts the power of the chick to pick up grains of corn without some preliminary lessons. On this point, he says, further experiments are needed. Such experiments have been since made by Mr. Spalding, aided, I believe, in some of his observations by the accomplished and deeply-lamented Lady Amberley: and they seem to prove conclusively that the chick does not need a single moment's tuition to teach it to stand, run, govern the muscles of its eyes, and peck. Helmholtz, however, is contending against the notion of pre-established harmony; and I am not aware of his views as to the organization of experiences of race or breed.

over" is spoken of, and when these processes are associated with "the modification of an organism by its environment," the same parallelism, without contact, or even approach to contact, is implied. There is no fusion possible between the two classes of facts—no motor energy in the intellect of man to carry it without logical rupture from the one to the other.

Further, the doctrine of evolution derives man, in his totality, from the interaction of organism and environment through countless ages past. The human understanding, for example—the faculty which Mr. Spencer has turned so skilfully round upon its own antecedents—is itself a result of the play between organism and environment through cosmic ranges of time. Never surely did prescription plead so irresistible a claim. But then it comes to pass that, over and above his understanding, there are many other things appertaining to man whose prescriptive rights are quite as strong as that of the understanding itself. It is a result, for example, of the play of organism and environment that sugar is sweet and that aloes are bitter, that the smell of henbane differs from the perfume of a rose. Such facts of consciousness (for which, by the way, no adequate reason has ever yet been rendered) are quite as old as the understanding itself; and many other things can boast an equally ancient origin. Mr. Spencer at one place refers to that most powerful of passions—the amatory passion—as one which, when it first occurs, is antecedent to all relative experience whatever; and we may pass its claim as being at least as ancient and as valid as that of the understanding itself. Then there are such things woven into the texture of man as the feeling of awe, reverence, wonder—and not alone the sexual love just referred to, but the love of the beautiful, physical, and moral, in nature, poetry, and art. There is also that deep-set feeling which, since the earliest dawn of history, and probably for ages prior to all history, incorporated itself in the religions of the world. You who have escaped from these religions in the high-and-dry light of the understanding may deride them; but in so doing you deride accidents of form merely, and fail to touch the immovable basis of the religious sentiment in the emotional nature of man. To yield this sentiment reasonable satisfaction is the problem of problems at the present hour. And grotesque in relation to scientific culture as many of the religions of the world have been and are—dangerous, nay, destructive, to the dearest privileges of freemen as some of them undoubtedly have been, and would, if they could, be again—it will be wise to recognize them as the forms of force, mischievous, if permitted to intrude on the region of *knowledge*, over which it holds no command, but capable of being guided by liberal thought to noble issues in the region of *emotion*, which is its proper sphere. It is vain to oppose this force with a view to its extirpation. What

we should oppose, to the death if necessary, is every attempt to found upon this elemental bias of man's nature a system which should exercise despotic sway over his intellect. I do not fear any such consummation. Science has already to some extent leavened the world, and it will leaven it more and more. I should look upon the mild light of science breaking in upon the minds of the youth of Ireland, and strengthening gradually to the perfect day, as a surer check to any intellectual or spiritual tyranny which might threaten this island, than the laws of princes or the swords of emperors. Where is the cause of fear? We fought and won our battle even in the Middle Ages: why should we doubt the issue of a conflict now?

PROGRESS OF MICROSCOPICAL SCIENCE.

The Histology of Leucocythæmia.—In Virchow's 'Archiv'* occurs an admirable paper by Professor Bollinger, of Zürich, which has been fully translated by the 'Medical Record' (June 3rd), and which we give here in part. He says that in the spleens of dogs there occur with unusual frequency—according to his observations, made alike on healthy and on diseased animals, in at least 10 per cent. of all dogs—true lymphomata. It is extremely probable that these form the starting point of leucocythæmia: in the case already related the splenic nodules were certainly present at the beginning of the disease. In confirmation of this assumption, he can bring forward a case observed in a dog, affording an instance of leucocythæmia in the incipient stage, which rarely comes under observation.

A very large old male dog was brought to the veterinary school in this place to be killed. It did not show any special signs of disease, and shortly before death ate abundantly with good appetite. The body was examined on May 6, 1871.

There were splenic leucocythæmia, and a large lymphoma of the spleen. The relation of the white to the red blood-corpuscles in the general circulation was 1 to 30 or 40; in the blood of the splenic vein, 1 to 10 or 15.

The animal was rather thin. The lungs were of normal extent, rather emphysematous, and strongly pigmented. On the left side were some subpleural hard bodies as large as pins' heads, which on microscopic examination were found to be bony deposits. The air-passages were normal. There was no remarkable change in the heart. Above the aortic valves, near the mouth of the coronary arteries, there was a prominence of the size of a lentil, with a rough surface, and mostly calcified: around it was distinct atheromatous thickening of the inner coat. On opening the abdominal cavity, a tumour nearly as large as a child's head was observed; it was covered by the great omentum, lay in front of the left kidney, and proceeded from the posterior surface of the upper part of the spleen. While the spleen itself was in other parts normal, containing little blood, of a pale flesh-red colour, and moderately firm, the tumour was of soft elastic consistence, of a shining dark-violet aspect, with the peritoneal investment much distended and at several points closely adherent to the omentum. On section, the tumour was found to consist of a spleniform dark brown-red tissue of slight consistence, having imbedded in it numerous deposits, mostly miliary, partly whitish, partly grey and diaphanous; they differed in no respect from the normal Malpighian bodies. In some parts towards the interior, these greyish-white deposits predominated so much over the remaining tissue as to become confluent. On microscopic examination, the whole tumour showed all the elements of the normal spleen; but in place of the fine Malpighian

* Band lix., Hefte 3 and 4.

bodies, there were large masses of lymphoid substance of similar structure. The spleen itself was remarkable for its great richness in granular blood-colouring matter and cells containing blood-corpuscles, as well as for a great amount of small and large fat-drops. The blood of the splenic vein was fluid, and of a clear red colour, and contained about one white corpuscle to ten or fifteen red ones. There was a slighter increase of the white corpuscles in the blood of the coronary veins of the heart, where the proportion was about 1 to 30 or 40. The liver was of a pale coffee colour, of normal size, and rather anæmic. The kidneys were of usual size; the capsule was easily removed, and on the surface beneath it were some cicatricial contractions. The substance of the kidneys was of a clear brown colour, and fragile. On microscopic examination, there was found to be marked fatty degeneration of the epithelium of the urinary tubules. The bladder was full of clear yellow urine, having a neutral reaction, and throwing down a flocculent deposit of albumen on being heated. On microscopic examination, instead of the cylinders that were expected, there were found numerous round nucleated cells, having the appearance of colourless blood-corpuscles, and a few spermatozoa. The mucous membrane of the bladder and urethra was normal. The stomach and intestine did not present any remarkable change. There was no enlargement of the lymphatic glands anywhere.

This case of simple splenic leucocythæmia, in which, besides the large lymphadenoma of the spleen, there was only a moderate increase of the colourless blood-corpuscles, can, without doubt, be applied in the direction indicated above. The disease was seized in its early stage, before it had gone on to metastasis, and to leucocythæmic disease of other organs. The remarkable amount of colourless blood-corpuscles in the urine, while the urinary passages were in a normal state, may perhaps be attributed to escape of the too abundantly formed cells into the diseased renal tissue; yet the simultaneous occurrence of spermatozoids in the urine indicates that there might possibly have been another source for these cells.

A further case of lieno-lymphatic leucocythæmia in the dog has been observed by Siedamgrotzky, of Dresden;* so that, in all, three cases of leucocythæmia in the dog have been fully described. The essential features of the case referred to were the following:—

A four-year old spaniel, which had suffered for some time from loss of appetite and from diarrhœa, died four days after admission into the veterinary hospital. A large firm tumour had been detected in the abdomen by palpation. At the necropsy, the spleen was found to weigh about 2¾ lbs.; it was much enlarged, and was covered with flat projections on the surface. All the lymphatic glands, especially the mesenteric, were remarkably enlarged; as were also the tonsils. The proportion of white to red corpuscles in the blood was 1 to 15. There were sanguineous effusions in the spleen, on the pericardium, in the mucous membrane of the tonsils, and on the gums.

Siedamgrotzky also describes a slight degree of lieno-lymphatic leucocythæmia as having been found in a cat which had died of internal

* Bericht über das Veterinärwesen im Königreich Sachsen für das Jahr 1871.

hæmorrhage. There was remarkable hyperplasia of the lymphatic glands, and the spleen was doubled in size.

With regard to other animals, there are observations on the occurrence of leucocythæmia in pigs and horses, to which he briefly refers.

In the pig, the following three cases have been described.

Leisering* relates a case of leucocythæmia in a pig, whose spleen, liver, mesenteric glands and blood, showed corresponding changes. Further details are wanting respecting this case, which was the first observed, and was evidently one of lieno-lymphatic leucocythæmia. Fürstenberg† describes a case of leucocythæmia in a pig, with enlargement of all the lymphatic glands, the spleen, and the liver. The spleen was more than 2 lbs. in weight; the liver, which was studded with leucocythæmic deposits, weighed more than 8 lbs. There was deposition of white corpuscles in the marrow of the bones. The blood was of a clear chocolate colour; the white corpuscles were enormously increased, their proportion to the red being 2 to 1.

In the case of splenic leucocythæmia in the pig to which he referred at the beginning of this article, the spleen was much enlarged, weighing 3½ lbs. There was remarkable enlargement of the kidneys, with leucocythæmic infiltration and extensive hæmorrhages. Leucocythæmic deposits were found in the liver and lungs. There was increase of the white blood-corpuscles, their proportion to the red being 1 to 5. The blood was of a clear red colour and watery. Whether the lymphatic glands were affected could not be ascertained. Microscopic examination of the hardened organs gave the following result. The spleen was in a state of hyperplasia, and its tissue presented exactly the same characters as in splenic leucocythæmia in man. Besides the abundant deposit of lymph-cells, there were great increase and thickening of the normal elements of the spleen. In the lungs, the chief seat of the leucocythæmic proliferations was the connective-tissue sheaths of the arteries and bronchial tubes. The liver not only showed a remarkable lymphoid deposit in the connective tissue between the acini, but also in the acini themselves there was so great a deposit of lymphoid cells, that their number exceeded that of the liver-cells. Finally, the kidneys were enlarged to more than double the normal size, and were studded with hæmorrhages; they contained so many lymph-cells that, under the microscope, the organ presented the appearance of a lymphatic gland, the remains of the normal kidney being discoverable at points only in the form of urinary tubules and Malpighian bodies.

A number of cases of leucocythæmia in the horse are recorded in literature; but I pass them over, as I have not been able to convince myself that they were cases of idiopathic leucocythæmia. In the meantime, they all might, with greater justice, be classed among those symptomatic and transient forms of leucocythæmia which, following Virchow, we designate leucocytosis. Considering the known irritability of the lymphatic system in the horse, and the corresponding liability in this animal to inflammatory affections of the lymphatic

* Bericht über das Veterinärwesen im Königreich Sachsen, 1865.

† Berliner Klinische Wochenscrift, 1870.

vessels and glands, it may be supposed that such leucocytoses, which consist in a temporary increase of the colourless blood-corpuscles, are of frequent occurrence; and this is indeed observed in a host of inflammatory and other diseases affecting the horse, such as glanders, farcy, &c. After large blood-lettings, the increase of white blood-corpuscles in the horse may go so far, that the coloured and colourless corpuscles appear nearly equal in number.

Counting the Blood-corpuscles in cases of Transfusion.—M. Brouardel gives, according to the 'Medical Record,' an interesting report on a case of transfusion of blood in an individual dying of prostration from incoercible vomiting after swallowing sulphuric acid. 150 grammes of blood not defibrinized, taken from his house-surgeon, M. Landouzy, were injected into the vein of the arm. The immediate consequences were favourable, but in twenty-six hours a relapse occurred, and the patient died with hepatization of the lower lobes of the lung. The necropsy showed ulceration of the pylorus. This observation showed this important point, that the application of M. Malassez's new method of numeration of the blood-corpuscles * has allowed it to be ascertained that a rapid destruction of the elements of the blood occurs when the individual cannot repair the incessant losses of the economy; while, in an individual in good health, repair is rapidly effected. Thus the patient had 3,200,000 red corpuscles in the cubic millimètre of blood; after the injection of 150 grammes of blood the figure was raised; but thirty hours afterwards it was again at the previous figure of 3,200,000; while in M. Landouzy, who had lost 300 grammes of blood, the number of blood-corpuscles before the bleeding was 4,300,000, immediately after it 4,000,000, and twelve hours afterwards 4,100,000. M. Dujardin-Beaumetz related a case of obstinate anæmia, in which transfusion produced a temporary benefit, as in the above case, and was twice repeated when that effect had passed off; the amelioration after the third transfusion was, however, of very brief duration, and the patient died on the following day. The results of the enumeration of the corpuscles mentioned above, give a key to the transitory effects of transfusion in these cases.

The Development of the Lobster.—A very good paper, which originally appeared in the Transactions of the Connecticut Academy (vol. vii.), is abstracted in the 'American Naturalist' for July, 1874. It is by Mr. S. I. Smith, Assistant in the Sheffield School, New Haven, U.S.A. It seems that the season at which the female lobsters carry eggs varies much on different parts of the coast. Mr. Smith states that lobsters from New London and Stonington, Conn., are with eggs in April and May, while at Halifax he found them with eggs, in which the embryos were just beginning to develop, early in September. The writer says that he has seen them in Salem with the embryos ready to hatch in the middle of May, and has been told by Mr. J. H. Emerton that they also breed here in November. It is not impossible that they breed at intervals throughout the year. This is an impor-

* 'London Medical Record,' January 8, 1873.

tant point. At any rate there should be a close time on the coast of New England, during April and May, and October and November. Persons should also be fined heavily for selling lobsters with eggs attached.

He divides the larval condition of the lobster into three stages. The first is a little under a third of an inch long, and was found early in July at Wood's Hole, Mass. In the second stage, the animal has increased in size, and rudimentary appendages have appeared upon the second to the fifth segments of the abdomen. In the third stage the animal is about half an inch long, and has begun to lose its Mysis-like (Schizopodal) appearance, and to assume some of the features of the adult.

There are probably two succeeding stages before the adult form is attained; one is described by our author, while the first of the two he supposes to have existed, but has not yet discovered. After this the animal ceases to swim on the surface, and late in summer seeks the bottom. They feed on the young of various animals, the larvæ of their crustacea, and when much crowded in captivity, on one another, the stronger devouring the weaker. In the first stage of the adult form, when the animal is about three-fifths of an inch long, it still differs from the adult so much that it would be regarded as a distinct genus. "In this stage, the young lobsters swim very rapidly by means of the abdominal legs, and dart backwards, when disturbed, with the caudal appendages, frequently jumping out of the water in this way like shrimp, which their movements in the water much resemble. They appear to live a large part of the time at the surface, as in the earlier stages, and were often seen swimming about among other surface animals. They were frequently taken from the 8th to the 28th of July, and very likely occur much later." Mr. Smith thinks the young pass through all the stages he describes in the course of a single season. Those in the last stage mentioned he believes had not been hatched from the eggs more than six weeks, and very likely a shorter time. How long the young retain their free swimming habit after arriving at the lobster-like form, was not ascertained.

Specimens three inches in length have acquired nearly all the characters of the adult. The descriptions of the different stages are very detailed, and accompanied by admirable figures.

"Of all the larval stages of other genera of crustacea of which I have seen figures or descriptions, there are none which are closely allied to the early stages of the lobster. Astacus, according to Rathke, leaves the egg in a form closely resembling the adult, the cephalo-thoracic legs having no exopodal branches, and the abdominal legs being already developed. Of the early stages of the numerous other genera of Astacidea and Thalassinidea scarcely anything is known, but as far as is known, none of them appear to approach the larvæ of the lobster. Most of the species of Crangonidæ and Palæmonidæ (among the most typical of Macrourans), of which the development is known, are hatched from the egg in the zoëa stage, in which the five posterior pairs of cephalothoracic appendages, or decapodal legs, are wholly wanting, as are also the abdominal legs, while the two anterior

pairs of maxillipeds, or all of them, are developed into locomotive organs. In no period of their development do they have all the decapodal legs furnished with natatory exopodal branches. There are undoubtedly larval forms closely allied to those of Homarus in some of the groups of Macrourans, although they appear to be as yet unknown.

"Notwithstanding these larval forms of the lobster seem to have no close affinities with the known larvæ of other genera of Macrourans, they do show in many characters a very remarkable and interesting approach to the adult Schizopoda, particularly to the Mysidæ. This appears to me to furnish additional evidence that the Schizopods are only degraded Macrourans much more closely allied to the Sergestidæ than to the Squilloidea."

What is a Sponge?—This seems a more difficult question than ever it has been. Haeckel has recently confirmed in great measure Mr. Carter's view, that it is a collection of Amœbæ-like infusoria, living among a framework of silicious or limestone spicules. A little later than Carter, the lamented Professor H. J. Clark, of America, published, in 1866, a paper in which he maintained that the sponge was an aggregation of flagellate infusoria, like monads of the genera *Monas*, *Anthophysa*, *Codosiga*, &c. The sponge, then, in his view was a compound protozoan animal. Now Haeckel contends that these monads of Clark are simply cells lining the general stomach-cavity of the sponge, each bearing a cilium or thread, and that the sponge is not a compound infusorian, but a much more highly organized animal related to the radiates, such as the Polyps (*Hydra*, &c.). He distinguishes in them a general cavity or stomach, the walls of which consist, as in the Acalephs, of two layers (entoderm and exoderm) of cells. He regards the sponges and Acalephæ as having been evolved from a common ancestor, which he terms *Protascus*.

Quite recently the editor of the 'American Naturalist' has received a paper by Metschnikoff on the development of a calcareous sponge (*Sycon ciliatum*). He clearly proves that Haeckel's view of the structure of the sponges was correct, but shows that there is no real relationship between the sponges and radiates.

Tokelau Ringworm and its Fungus.—Dr. T. Fox has given in the 'Lancet' (Aug. 29) an interesting paper on the above subject. He says that there is a form of eruption which appears to be very common at Samoa, the hitherto unknown cause of which he has recently been able to discover; and he seeks this opportunity of placing the main facts in regard to it before the profession. The Rev. Dr. Turner, M.D., refers to the disease, in his First Annual Report of the Samoan Medical Mission, under the term "Tokelau Ringworm," or "Lafa Tokelau"; so named from its having recently been introduced to Samoa from Tokelau or Bowditch Island. Dr. Turner says, "It is a scaly disease, much more like ichthyosis in its general appearance than any other disease with which I am acquainted. The scales, however, differ from those of ichthyosis, in that they are not disposed in squares. They run in concentric circles, and may be well represented by taking a sheet of stout cardboard and shaving the upper layer of it

in such a way as to make it coil up in circles. The rings of the desquamated cuticle are about a quarter of an inch apart. . . . My impression is that it is a parasitic disease, but as yet I have not succeeded in discovering any parasite; nor can I speak definitely of any treatment which has proved successful." It seems that the existence of the disease was noticed by the officers attached to the United States' exploring expedition, under the command of Captain Wilkes, in 1841, who noticed it in the Kingsmill group, and spoke of it under the designation of "Qune," and as, at some of its stages, *resembling* the ringworm.

"Dr. Mullen, of H.M.S. 'Cameleon,' has," says Dr. Fox, "been good enough to forward me some facts about the disease, through the courtesy of the Director-General of the Navy Medical Department. He remarks that Dr. Turner has noticed, about three hours after the application of sulphur ointment to the skin, some winged insects bursting through the ointment and flying away. 'On scraping the skin there were perceived dipterous insects, somewhat smaller than midges, others still smaller, and what appeared to be the dipterous insects in the pupa stage. Now these are not accidental accompaniments, for they have been found in all cases about three hours after the ointment has been applied; and the Rev. Dr. Turner has procured "scrapings" from missionaries of other islands, who, by his advice, have used the ointment, and has always found the same insects. It is strange,' Dr. Mullen continues, 'that before applying the ointment no trace of these insects, nor any pustules, papules, &c., indicating the presence of such large parasites, can be discovered. Possibly they may exist as ova in the under surface of the scales, which become developed on the application of the ointment; but is not this development too rapid even for the insecta?'

"I have received 'scrapings' from the skin and a number of the dipterous insects referred to in the above paragraph from Dr. Turner; and I now proceed very briefly to summarize the conclusions to which I have come after a careful examination of them.

"The disease is clearly a form of ringworm (tinea), dependent upon the growth, amongst the cuticle cells, of a vegetable fungus. The general features of the disease, in its mode of onset, its progress, symptoms, and naked-eye characters, are those of an exaggerated tinea unquestionably. There are points of difference, I admit; but I will refer to these in a moment. In the 'scrapings' of cuticle I find abundant evidence of a vegetable fungus of a most luxuriant kind. *This fungus exists in great abundance; but, though so plentiful, its presence may readily be overlooked, unless a very thin layer of the 'scrapings' is examined.* Of the accompanying illustrations, one of the figures [which we regret we do not possess] represents the fungus magnified under a power of 500 diameters, and as drawn with the camera. It will be noticed that the fungus elements are very large. They bear, indeed, a resemblance to the parasite of the so-called *Eczema marginatum* of the Germans. I am not at present prepared to say whether the fungus is a modification of the trichophyton, or a new and special one. I await further experiments, and do not propose

therefore to give the fungus or the disease a new name. The second figure shows the fungus as seen under a power of 1500 diameters, and conveys a very good idea of the structure of the cell-wall of the conidia, and the mode in which these conidia are developed within the cell-wall of the mycelial threads. But I purposely omit entering into any further account of the microscopic history and relationship of the fungus, as my object is a practical one. Suffice it, that I have discovered the fungus and figured its main features.

"I have been unable to detect any dipterous insects—of which I have specimens—in the 'scrapings' which I have examined; and it is clear to me that their presence is accidental, and that they are attracted to the skin, in Tokelau ringworm, by the ointments applied to it, and in which they become imbedded. They are not, so far as the microscope enables me to judge, present in the diseased skin until after ointments have been applied. Further, dipterous insects could not, I take it, possibly cause such an eruption as Tokelau ringworm; and it is impossible to suppose that, in or upon a skin in which not a trace whatever of their presence exists, the application of a strong parasiticide would cause the rapid development, in the space of three hours, of a host of these dipterous insects from ova supposedly existing in the skin, and undiscoverable by accurate means of detection. I presume it is the fact of the non-discovery of the fungus which led to the supposition that the diptera may be the cause of the disease. But, now that I have demonstrated the presence of the fungus, and having regard to the general features of the eruption in Lafa Tokelau, the aspect of the question of the relationship of the diptera to the disease, in the light of cause and effect, is altogether altered. I have said that, as compared with exaggerated tinea circinata, Tokelau ringworm offers some points of difference. I think these do not refer to essential features of the eruption, but rather to those which are accidental—viz. to the infiltration and the scaliness; and these differences are to be explained, I think, by the greater luxuriance and amount of fungus present, which necessarily cause a greater degree of inflammation. It is not necessary to suppose that the fungus is a special one; the differences referred to will be equally accounted for if it should turn out that the parasite is a modification—a more luxuriant form than usual—of the trichophyton."

CORRESPONDENCE.

GUNDLACH'S $\frac{1}{8}$ AND BENÉCHE'S No. 7.

To the Editor of the 'Monthly Microscopical Journal.'

DENSTONE COLLEGE, *August* 24, 1874.

SIR,—With our brother microscopists and opticians in Germany a favourite method of testing a $\frac{1}{8}$-inch objective is to try if it will show the lines on *P. angulatum* with perfectly straight sunlight, using, of

course, as the German fashion is, both mirror and diaphragm; and their conclusions as to the quality of the objective under examination are formed accordingly.

For my own part, I have for some time adopted a proceeding somewhat similar, but infinitely more trying. Instead of straight sunlight, I employ the straight light of a common composite candle, and discard both mirror and diaphragm.

My $\frac{1}{8}$ inch (one of Gundlach's earliest issues) will in this way not only do all that I have seen German eighths do, with the help of sunlight, mirror and diaphragm, but do it with greater sharpness and distinctness. I may mention that, when in the summer of 1872 I visited Messrs. Seibert and Kraft's establishment at Charlottenburg, and Herr Seibert showed me the above-mentioned feat with sunlight, as a grand *tour de force*, I took my own $\frac{1}{8}$ inch out of my pocket, and requested Herr Gundlach, who chanced to enter the room at that moment, to try it against Herr Seibert's. He did so; and the result was a complete victory for my glass, to Gundlach's great delight, when I informed him that the glass was one of his own manufacture.

So much for eighths.

Well, on Saturday last, L. Benéche of Berlin sent me, as I had requested him, a specimen of his newly improved No. 7, which corresponds to a weak English $\frac{1}{4}$ inch.

I tried it the same evening, using straight candlelight and a B eyepiece, but without mirror or diaphragm. The test applied was one of Möller's slides of *P. angulatum*.

Under these conditions it showed the markings on *P. angulatum* with the utmost distinctness, leaving nothing to be desired on that score. Indeed, I preferred its performance to that of my $\frac{1}{8}$ inch, with which I compared it.

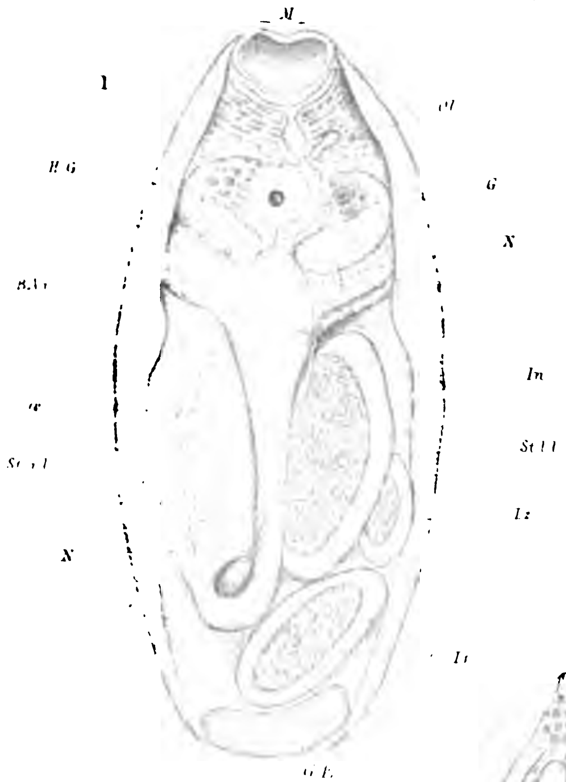
I then took out the eye-piece, and used as an eye-piece a very indifferent 1-inch objective. This method, though resulting in a perceptible loss of light, left the definition as sharp as ever. My $\frac{1}{8}$ inch, on the other hand, broke down completely under this last test.

Then I proceeded to try Benéche's glass on a variety of other tests, from *P. balticum* up to *S. gemma*, and in all cases with excellent results. Its performance on *S. gemma* I shall leave unrecorded, as the truth here would seem incredible. Meanwhile, its performance on *P. angulatum* is a pretty "big thing" for a $\frac{1}{4}$ inch; and I shall be glad to hear if any of your readers can do the same with *their* quarters.

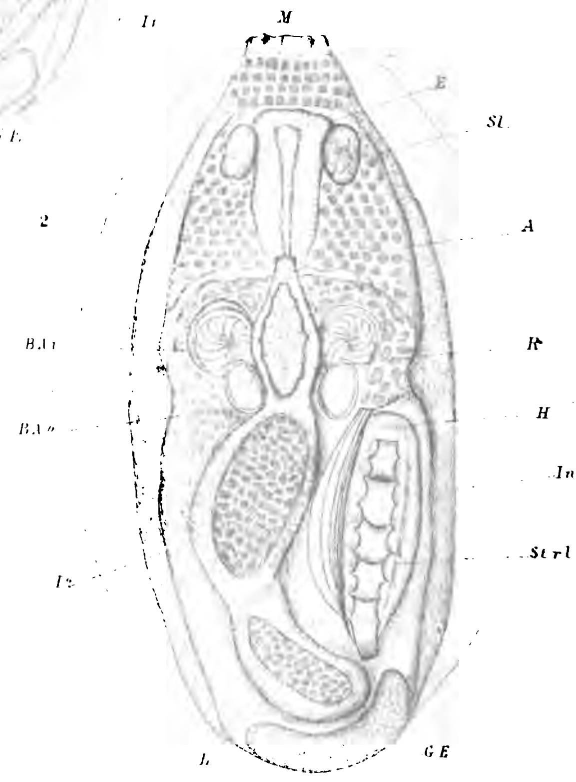
I am yours, &c.,

W. J. Hickie.

1



2



Neural and Haemal views of Oikopleura

THE

MONTHLY MICROSCOPICAL JOURNAL.

NOVEMBER 1, 1874.

I.—*Supplementary Remarks on Appendicularia.*

By Alfred Sanders, M.R.C.S.

(*Read before the* Royal Microscopical Society, *October* 7, 1874.)

Plate LXXX.

At the beginning of this year I had the honour of laying before this Society some remarks on two apparently new species of Appendicularia, belonging respectively to the genera Oikopleura and Frittillaria; a much larger number of specimens of the former, and a longer time for examination, have enabled me, during the course of this summer, not only to correct some misinterpretations of structure, but also to add several details to my former paper.

I have, unfortunately, not been able to meet with further specimens of the species of Frittillaria which I then described, so that my remarks will be entirely confined to the genus Oikopleura on the present occasion.

The species of this genus, the description of which I am now about to complete, abounds on the south coast of England; I have found specimens in large quantities at Weymouth, Cowes, and Newhaven, and it is to be presumed that they occur also in the sea between those places.

They are best caught in a muslin net left for a short time in the tideway a little before high water, at about the period of the spring tides; the plan suggested by Dr. Fol, of bailing them out

EXPLANATION OF PLATE LXXX.

Fig. 1.—Neural view of Oikopleura.
„ 2.—Hæmal „ „
The tail is not represented, and is supposed to be cut off.

A, Anus.
B, Ae, External branchial aperture.
B, Ai, Internal „ „
E, Endostyle.
G, Ganglion.
G, E, Generative gland.
Gl, Glandular bodies attached to the endostyle.
H, Heart.
H, G, "Haus" gland.
I, 1; I, 2; Intestine.
In, Integument.
M, Mouth.
N, Nerve.
O, Otolithe.
Œ, Œsophagus.
Ol, Olfactory organ.
R, Rectum.
St, Rl, Right lobe of stomach.
St, Ll, Left „ „

into a large glass vessel without lifting the net entirely out of the water, answers very well.

There appears to be some confusion with regard to the nomenclature of the position of the body; Dr. Fol * and also Vogt † term the side on which the nervous system occurs, the dorsal, and the side to which the tail is attached, the ventral. This would be correct in regard to the development of the Ascidia in reference to the vertebrate type of structure. In Professor Huxley's ‡ paper, on the other hand, the nervous system is described as being on the ventral side. In order to avoid this confusion, perhaps it would be better to call the side on which the ganglion is situated the neural, and that to which the tail is attached the hæmal; and in speaking of the direction of any part, whether towards the neural or hæmal side, the corresponding terms § would be neurad or hæmad. But there still remains the necessity, when referring to the right or left side of the animal, to determine whether the neural side is to be considered ventral or dorsal; but as in my last paper I followed Professor Huxley's nomenclature in reference to this aspect, I think it will be preferable still to do so, and in order to distinguish the right from the left side, to speak as if the nervous system were situated on the ventral side of the body. This discussion may appear trivial, but it will be found that the present paper would be unintelligible without some definite rule on the subject.

It is comparatively easy to obtain a side view of these animals, because, being narrower in that direction than from the neural to the hæmal side, they naturally fall into that position when they are slightly confined by the covering glass. But to get a hæmal or neural view is much more difficult; in this case it is necessary so to arrange the covering glass that it does not touch any part of their body, and being thus free to move about as they please, a good view is a matter of chance and patience. The plan that I have found to answer best, is to support both ends of the cover by a sufficient thickness of paper, to put the animal into a very small drop of water, and by means of a horsehair to draw this drop of water into a point; when this is done they sometimes drive themselves into the inlet thus formed, and revolve with great rapidity round their long axis; but after a time they become tired, and remain quiet for a few seconds, when it is possible to gain a view of them in either the hæmal or neural position. The specimens that I examined varied in length of body from 0·24 mm. to 0·72 mm.

Integument.—The whole body, with the exception of the extreme posterior end, is surrounded by a hyaline membrane; that this is

* 'Mem. de la Société de Phys. et de l'Histoire Naturelle de Genève,' tom. xx., 12me partie.

† 'Mém. de l'Institut National Genevois,' tom. ii.

‡ 'Quar. Jour. Mic. Science,' vol. iv., 1856.

§ Derived from Vertebrate Anatomy.

not the commencement of the structure known as the "Haus" I am convinced, because, although I have only met with this structure in a fully formed condition once or twice, I have frequently seen what appears to be its beginning, and in each case it was separate from and covered by the hyaline substance; be that as it may, all the specimens that I have examined possessed this glassy covering.

Beneath this, the body is enclosed in thick granular parietes, which extend from each side of the generative gland forward to the entrance of the pharynx; the deficiency of this granular wall at the posterior end of the body is filled up by a thin membrane, to which the generating gland adheres. The interior of the parietes is lined by what appears to be an epithelial layer, which presents the appearance of variously shaped cells at different portions of the body, but I am not decided whether this layer is a separate structure, or whether it is only the optical expression of the elements composing the thickened parietes, for when an optical section of the wall is obtained a distinct and separate layer of epithelium cannot be made out; surrounding the entrance to the pharynx, however, these quasi cells have a four-sided appearance; as seen from the hæmal side there are four rows of them, here they approach the form of a square; but on the neural side they resemble layers of bricks, each element forming a parallelogram with the angles rounded; behind these, in that part of the walls covering the endostyle on the hæmal side and the nervous ganglion on the neural side, they are circular in outline, being arranged in rows in such a manner that those of the second row occur opposite the interspaces of the first row, those of the third in like manner opposite the interspaces of the second, and so on. In some few cases these cells appear to be hexagonal in shape; when this occurs each hexagon is separated from its neighbour by a clear line when the focus is properly adjusted.

As soon as the process of dying begins to set in, these cell-like appearances are more distinct, the divisions between them become more marked, they swell out, and on a profile view project as rounded eminences into the somatic cavity; it is this appearance which perhaps gave Dr. Fol* the idea that the somatic parietes were composed of a single cellular layer, which he terms the ectothelium or epidermis; but in healthy specimens the walls of the body, as seen in an optical section, present only the appearance of indistinct molecules imbedded in a granular substance, with the above-mentioned cells forming an apparent lining membrane.

Belonging to the integumentary system is a large gland, which Dr. Fol declares to secrete the gelatinous material forming the so-called "Haus," which interpretation I am inclined to confirm

* *Loc. cit.*

from my own observations. This gland consists of two parts; anteriorly there are a pair of oval swellings of the inner surface of the integument, one being situated on each side of the ganglion and otolithic vesicle; these swellings gradually subside to the level of the integument towards the middle line in front, but more suddenly behind; they project nearly half-way across the body, and are composed of circular molecules imbedded in a matrix which appears to be composed of sarcode. When deterioration first begins to set in, a few rows of large hexagonal cells make their appearance on each side of the central nervous system, which, as the animal progresses towards dissolution, become more and more of a rounded form until they represent rows of hemispherical projections; it is in this state that Dr. Fol has given a figure of these glands. That this appearance is abnormal, and the result of commencing decomposition, is demonstrated by the fact, that it is not seen in perfectly fresh specimens, that it comes on gradually, passing first through the hexagonal stage, and that subsequently other parts of the integument show hemispherical projections resembling these, but of varying sizes, according to the position they occupy as mentioned above; during the farther progress of dissolution they develop into globules of a highly refracting appearance, which obscure all view of the interior of the body; in extreme cases a like process goes on in the walls of the viscera simultaneously.

The posterior portion of these glands presents quite a different appearance from the anterior; this part consists of three transverse rows of square corpuscles terminated anteriorly by a sharp edge; each of these corpuscles is divided from its neighbour by a clear line; this band is finished off posteriorly by a border of finely granular material, which is obscurely filled in some cases by corpuscles of an oval form, with the long axis placed transversely; it was this band of square corpuscles that I rashly concluded, in my last paper, were rows of stigmata, but that they are not openings, and have nothing to do with such structures, is easily shown by feeding these animals with indigo.

There is no excretory duct to these glands, but the gelatinous material of the "Haus" appears to be exuded through the finely granular portion which is situated between the two in the mid line on the neural side of the body.

The endostyle, and the glandular body on each side of it, may be considered as appendages of the integument. The endostyle, which was briefly described in my last paper, appears on the dorsal view to be a solid body very nearly divided longitudinally into two halves by a fissure, which is slightly expanded at its anterior end; its posterior extremity projects for a short distance beyond or posterior to the anus. A minute examination of its substance

discloses indistinct molecules; in many cases, the addition of aceti acid, besides giving it a coarsely granular appearance, causes a few transverse lines to become visible, as if it were about to divide into segments.

According to Dr. Fol, this body secretes a glairy mucus, which mixes with the food as it passes down the pharynx, and, therefore, belongs to the digestive system. I have not observed this secretion, neither have I seen the pair of styles with which the same authority remarks that the endostyle is provided.

There are two apparently glandular bodies, one on each side of the endostyle, instead of only one, as I supposed in my last paper, for reasons which will appear presently; in structure they so far resemble the endostyle as to show obscure indications of molecules in their substance; it is noticeable that in small specimens these bodies are larger in proportion to the size of the endostyle than in large specimens; perhaps they have some function in the economy of these animals analogous to certain fœtal structures in the vertebrata, e. g. the thymus; their position, also, it may be remarked, is homologous.

Four openings pierce the integument, but only two need be noticed at present; these are the ciliated branchial openings. In my last paper I mentioned that there existed only one of these openings; this mistake arose from the circumstance that all the specimens were examined on the side, this being the position which the animal invariably assumes when slightly held by the covering glass, and when this is the case, one branchial opening conceals the other (the same thing occurs in the case of the glands just mentioned). It so happened, in my investigations last year, that when the creature had been sufficiently examined in this position, it was out of my power to procure any further specimens, so that I was unable at the time to correct misapprehensions caused by want of variety in position. The branchial openings are situated on the hæmal side of the pharynx; they are rather long tubes, having a direction backwards, hæmad, and slightly towards the middle line. The internal openings are larger than the external; the former are circular in outline, and are provided with a circlet of strong cilia, which, when in action, gives a figure resembling the engine-turning on the back of a watch, or the drops of water from a revolving wheel. The circumference of these openings is enclosed by two or three strong circular fibres, outside of which a circlet of pentagonal cells occurs; each of these cells is provided with a clear spot resembling a nucleus; five of them can be counted on the anterior part of the circumference of the openings, which is all that is visible on the hæmal view of the animal.

The external openings are quite plain, and unprovided with cilia; they are smaller than the internal, somewhat oval in shape, and are

situated one on each side of and in close juxtaposition to the commencement of the rectum. These tubes are more visible in the largest specimens; indeed, in the smaller ones they are scarcely perceptible: this might account for my having missed seeing them in my former observations.

I tried feeding some vigorous specimens with indigo, and found that the particles entered the pharynx through the branchial apertures in a regular stream; the greater part went out again through the mouth, but the part which struck against the neural wall of the pharynx turned back, and were carried by the ciliary current into the œsophagus and thence into the stomach, and eventually into the remainder of the intestine; the indigo followed this course in every instance in which the experiment was tried.

Digestive System.—This species of Oikopleura is distinguished by the high development of its alimentary canal.

The general description of this tract was given in the former paper; I will content myself, therefore, in this place with adding some further details to that account.

The mouth looks towards the neural surface; it is heart-shaped, being provided with a rounded anterior lip.

The stomach is a complicated organ; it consists of two flattened disk-like lobes, one of which lies parallel to the right side of the body, the other lobe is situated at right angles to this on the neural side; it is this latter part that I termed the first portion of the intestine, from the fact that fæces begin to be observable therein; but as most authorities appear to consider it to be a lobe of the stomach, I shall adopt that view in future. The two lobes in *A. flabellum* are described by Professor Huxley * as being parallel, and not at right angles, as in this specimen. The right lobe of the stomach, which corresponds to the left lobe in Dr. Fol's nomenclature, is provided with two different species of cells in the lining membrane; one sort are large, and form hemispherical projections into the cavity of the stomach; these are arranged in a crescentic form, and occupy that part (about half) of the mucous membrane which is situated towards the hæmal side of the body; the space between the right and left walls on this side is occupied by four or five cells much larger than the remainder.

The other portion of the mucous membrane of the right lobe of the stomach is formed of flat irregularly polygonal cells, which occupy the spaces between the above-mentioned rounded cells, and also spread over that wall of this lobe which is directed towards the neural side; these flat cells vary in shape and size; they line the whole of the right lobe not occupied by the rounded cells. The transition between the right and left lobes is marked by a longitudinal ridge at the angle which they form together, but the

* *Loc. cit.*

neural wall of the two parts is continuous, without any mark of transition.

The left lobe is also lined by flattened cells, which form a tesselated or pavement epithelium. These cells are polygonal in shape, like the flat cells of the right lobe, but differ in each containing a clear round spot resembling a nucleus, the rest of the cell contents being coarsely granular. The shape of these cells varies in different specimens; in some the prevalent form is elongated, pointed at one end and broad at the other, at which part the clear spot is present; but in the greater number they are irregularly polygonal, with the clear spot at or near the centre.

A profile view of this part of the stomach shows that these cells are slightly elevated above the level of the walls of this viscus, so that the separation between them is formed by a series of channels, which are indicated in a front view by clear lines of demarcation.

The colour of these cells is generally darker than in those of the right lobe and of the intestine; in a few specimens they were of an orange tint, and in one only of a deep purple, the second part of the intestine being of a lighter shade of the same colour.

The pylorus is placed at the posterior wall of the left lobe, close to the angle between it and the right lobe, so that the greater part of this left lobe forms a *cul de sac*, which perhaps might be considered as a rudimentary liver; if so, this animal presents another point of resemblance to the amphioxus, inasmuch as it possesses a liver constructed in the form of a blind sac.

The last-mentioned organ being considered as belonging to the stomach, there only remain two chambers and the rectum as forming the intestine; the first and second of these (which I termed second and third portions of the intestine in my last paper) are oval chambers, which present no apertures except when the fæces are passing; both of them are lined by an epithelium formed of flattened cells, like that of the left lobe of the stomach; the cells of this epithelium differ, however, in not containing a clear spot in the centre, or when this is present, as happens in a few cases, it is so indistinct that it is easily overlooked and soon disappears entirely.

The rectum continues forwards from the second chamber and ends in the anus, which is situated on a distinct papilla in front of the insertion of the tail. This part of the intestine is lined by round cells which project into the cavity.

That part of the right lobe of the stomach which is lined by flattened epithelium is ciliated, as also is the whole of the left lobe, together with all the intestinal canal except the rectum.

Nervous System.—The nervous system of these animals is very highly developed, more so even than in their allies the ascidia.

The principal ganglion is situated to the left of the conspicuous otolithic vesicle, half the circumference of which it embraces; anteriorly it is prolonged by a continuation of the same substance, which on arriving at the posterior border of the mouth divides, in a fork-like manner, into two branches, which half surround the mouth between them. I did not see in these specimens the nerve which Dr. Fol declares unites the two ends of these branches over the hæmal edge of the mouth; the posterior end of this main ganglion terminates in an enlargement, which contains a body resembling a nucleus. The whole of this may be looked upon as one ganglion, as the nature of the material is the same throughout. Three principal nerves are given off from the posterior end of this ganglion, the external one on each side supplies the branchial apertures; the middle nerve passes down towards the posterior end of the body, first outside the pharynx and then between the œsophagus and the stomach, and reaching a depression between the two lobes of the stomach it turns towards the hæmal side of the body and joins the first ganglion of the tail; this ganglion is larger than any of the remaining ganglia of that appendage; it is elongated and fusiform, composed of several elements, and gives off several nerves to the muscles adjacent. The number of ganglia in the tail varies from nine to twelve; they are all of small size in comparison to the first, containing not more than two or three elements, and in some cases only one, which looks like a mere swelling of the commissural nerve.

I noticed a curious mode of termination of the nerves in the muscles of the tail in some specimens, which is also mentioned by Dr. Fol; each branch of some of these nerves, when it reaches the muscle, terminates in a little swelling. Whether this swelling was on the outside or the inside of the muscle I did not determine; according to Dr. Fol it was on the latter. They reminded me of the terminations of the nerves in Tardigrada, as described by Dr. Greef,* except that they were not prolonged along the surface of the muscle.

The organs of sense are two in number; first, the otolithic vesicle, which is a conspicuous spherical body containing one otolithe; secondly, a pyriform diverticulum situated immediately in front of the last; this is a hollow sac attached by its smaller end to the chief ganglion. It is hollow, and strongly ciliated within, the motion of the cilia resembling the flickering of a candle-flame. Dr. Fol describes it as the organ of smell, and mentions that it opens into the pharynx, but Vogt † did not see this aperture.

Vascular System.—The only part of the vascular system that is visible is the heart. The blood being colourless and totally

* Max Schultze, 'Archiv. für Mic. Anat.,' Erster Band, Erster Heft.
† *Loc. cit.*

devoid of corpuscles, unless some extraneous matter, such as zoosperms, becomes mixed with it, the vessels which convey it are invisible.

The heart is situated between the right lobe of the stomach and the second part of the intestine. It is composed of several longitudinal fibres which are attached anteriorly along a transverse fibre, which, passing immediately in front of the right lobe of the stomach, is fixed to the parietes of the body. Posteriorly they join together to form a single fibre, which passing behind this lobe is in like manner attached to the parietes at that point. When the generative gland increases in size it pushes between the right lobe of the stomach and the first part of the intestine, giving the appearance as if the posterior end of the heart were fastened to its enveloping membrane. I am rather uncertain whether these fibres are united together by a membrane, but appearances are more in favour of the idea that it is not present in this species; at all events, the wall next the stomach is deficient, neither is there a cell present at either end; Mr. Ray Lankester's opinion, therefore, that the heart is a mere churning organ is so far confirmed.

Generative Organs.—None of the specimens that I examined were sufficiently advanced to show fully-developed generative organs; in a few of the largest (0·72 mm. in length of body) this gland contains in its substance spherical bodies of a granular aspect, which only wanted a nucleus and nucleolus to resemble cells. They can be squeezed out, and then float about in the surrounding liquid; I should think that they are not ova, but rather sperm-cells.

In this paper I have given a figure of the neural and one of the hæmal side of the animal; I did not consider it necessary to repeat the figure of the side view given in my last paper, as it is quite correct, with the exception of the omission of a tube from the branchial aperture slightly backward and towards the hæmal side immediately in front of the stomach.

If one may judge from published figures, the animal which I have just described differs a good deal from all the other species of this genus; but I have refrained from giving it a special name, because, until we know to how much variation each species is subject, it would perhaps be rash to assume that the differences which this particular specimen shows are sufficient to form a true species. These differences from Dr. Fol's *O. Dioica*,* which presents the nearest approach to my specimens, are seen in the shape of the integument, which is not so angular, in the shape of the stomach, which is more rounded, and in the shape of the tail, which is more pointed at the extremity.

* 'Mém. de la Société de Phys.,' &c., pl. iv., figs. 1–6.

II.—*New Diatoms.* By F. Kitton, Norwich.

(*Read before the* Royal Microscopical Society, *October* 7, 1874.)

Plates LXXXI. and LXXXII.

Through the kindness of my friend Captain Perry, of Liverpool, I am enabled to introduce to the notice of the Fellows of this Society some new and rare forms of Diatomaceæ which I have detected in a dredging made by him off Navy Bay, Colon, Panama.

This gathering is one of the most interesting it has ever been my fortune to examine: in addition to the many fine forms of Diatomaceæ, Foraminifera and Polycystina are not of unfrequent occurrence. Silicified casts of the borings of some species of Cliona, the chambers of Foraminifera (often with the pseudopodal apertures), fragments of Echinus spines, and Bryozoa may be found among the heavier débris after the calcareous matter has been destroyed by acids.

The species of Diatomaceæ are numerous and fine; those which I am about to describe are, I believe, new.

Perrya, N. G., mihi.

Free, elongated, frustules compressed, sometimes slightly constricted, extremities rounded, striæ transverse moniliform. Marine.

This genus is distinguished from Nitzschia, its nearest ally, by the absence of a keel, and also by its very much compressed valve.

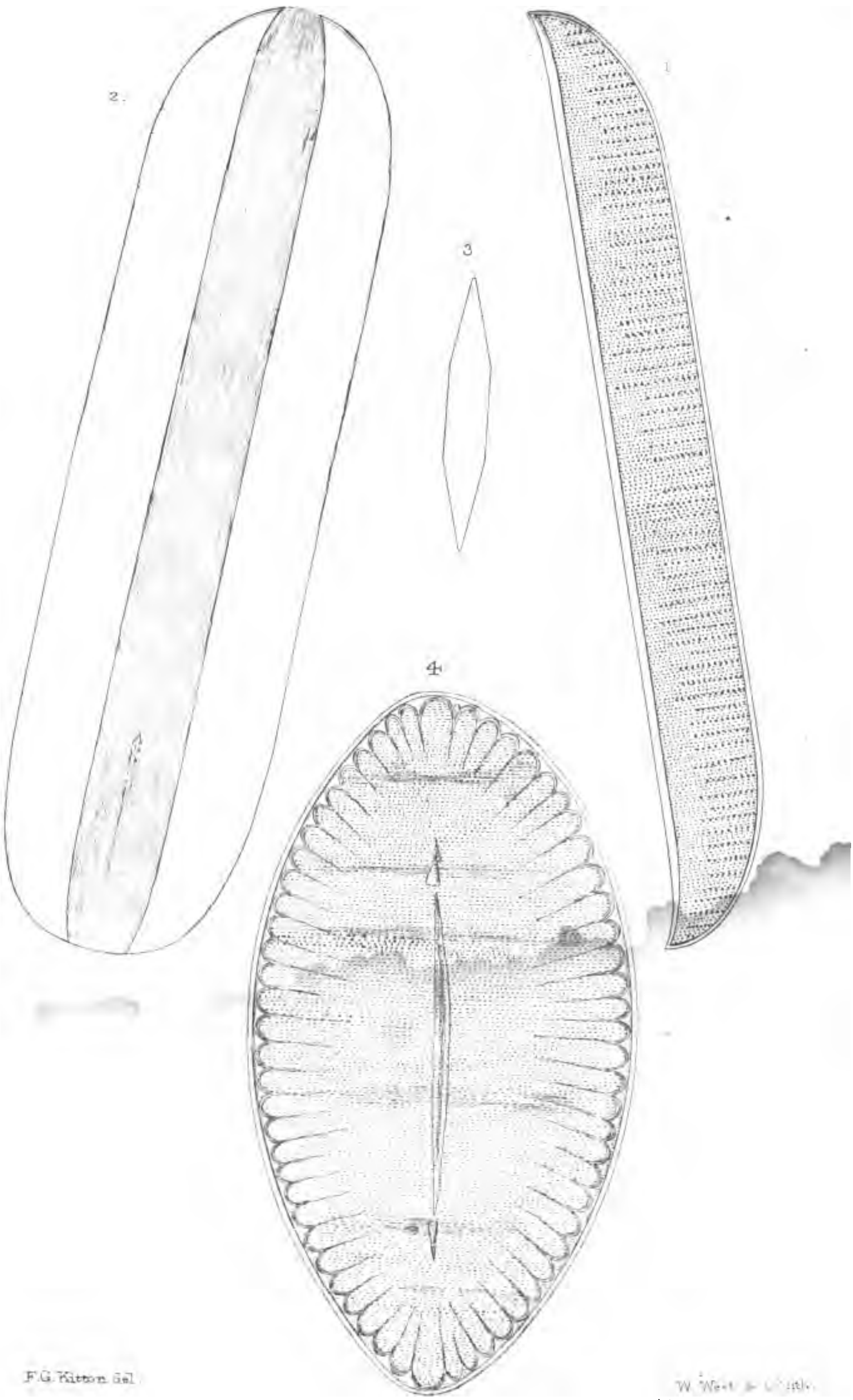
P. pulcherrima, F. K.—Valve in f. v. linear, inner or ventral margin straight or very slightly concave, outer or dorsal margin straight, suddenly rounded at the apices, markings, distant moniliform dots, in transverse series, not reaching ventral margin, between which are fine moniliform striæ, s. v. narrow, linear, suddenly tapering towards the acute extremities, a central line of large moniliform dots dividing the valve. Length ·0090″ to ·0200″, breadth ·0015″. Navy Bay, Colon, Panama; Campeche Bay. = *Nitzschia pulcherrima* Grunow, m. s. Herr Weissflog, in litt. Pl. LXXXI., Fig. 1, valve f. v.; 2, frustule; 3, ideal section of ditto.

This form somewhat resembles Nitzschia, but the compressed valve and absence of keel indicate that the resemblance is only

---

DESCRIPTION OF PLATES LXXXI. AND LXXXII.

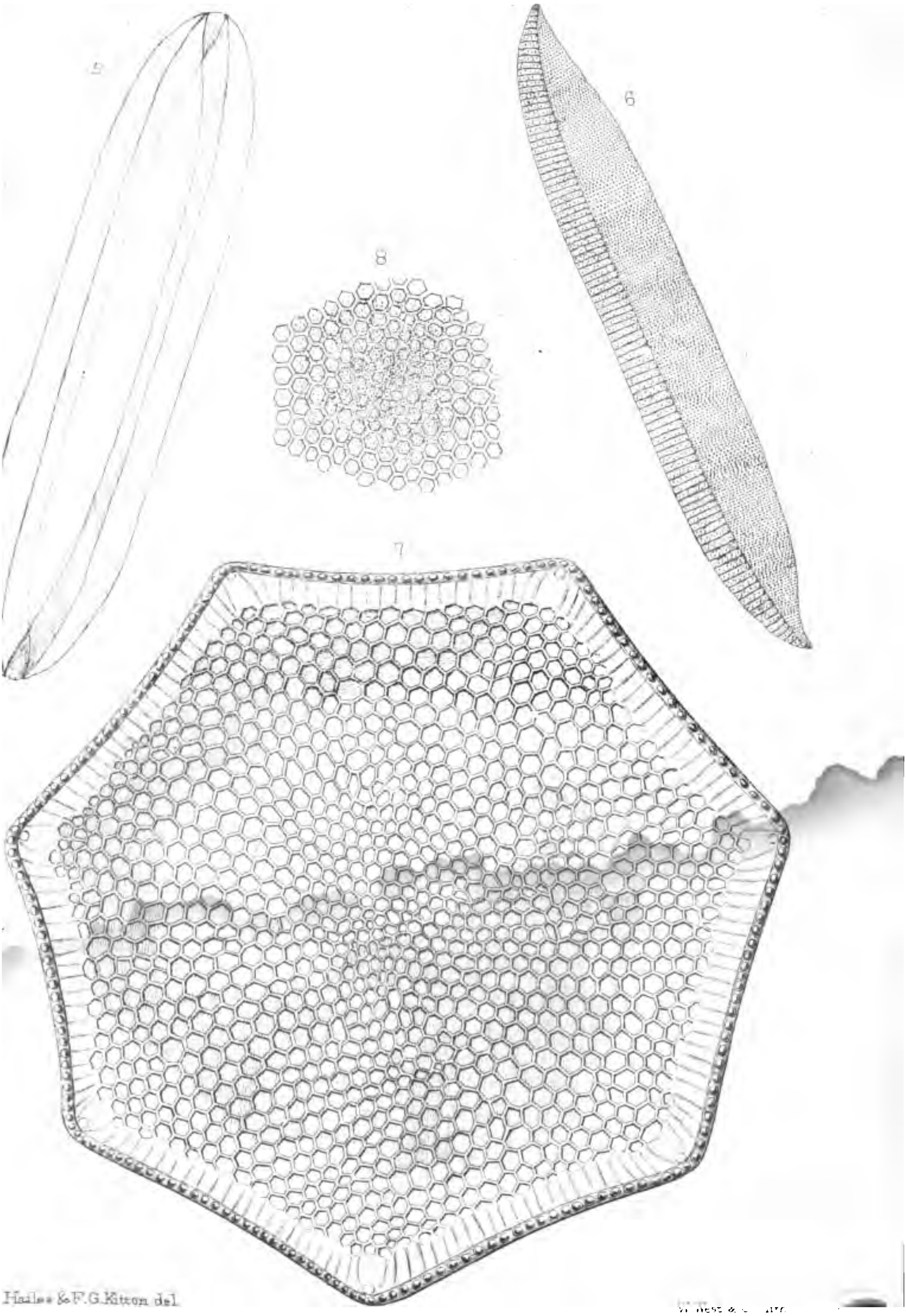
Fig. 1.—*Perrya pulcherrima*, valve.
„ 2.— „ „ frustule.
„ 3 — „ „ ideal section of ditto.
„ 4.—*Surirella contorta*, valve.
„ 5.—*Nitzschia grandis.*
„ 6.— „ „ outline of frustule.
„ 7.—*Triceratium favus*, var. *sept-angulatum.*
„ 8.—Portion of inner surface showing the punctate film.
All × 400 diameters.



F. G. Kitton del.

W. West & Co. lith.

New Diatoms.



Hailes & F. G. Kitton del.

W. West & Co. lith.

New Diatoms

superficial. It resembles Amphiprora in the f. v., but the absence of the sigmoid keel and central nodule distinguishes it from that genus.

I have detected two or three other species in the Colon and Campeche dredgings, which I hope to describe in a future paper.

Nitzschia grandis, n. sp., F. K.—Frustule linear, ends rounded, valve linear, suddenly tapering to the acute and incurved extremities, keel subcentral, costate punctate between the costæ, remainder of valve marked with distinct moniliform striæ in transverse series. Costæ 10 in ·001, striæ 25 in ·001; length from ·0100″ to ·0200″. Navy Bay, Colon, Panama. Pl. LXXXII., Fig. 5, valve; 6, outline of frustule, shaded portion the cingulum; the dotted line shows ventral margin of lower valve.

This form is perhaps one of the finest of the genus, and I know of no species with which it is likely to be confounded. *N. Brightwellii* equals it in size, but differs in the position of the keel and also in the striation.

Triceratium favus, var. *sept-angulatum*, F. K.—Valve large with seven slightly concave margins, processes produced, cells hexagonal somewhat irregular in size, centre of valve turgid, marginal cells elongated, margin with large moniliform granules, inner surface of valve punctato-striate, radiant, about 20 in ·001; extreme breadth of valve ·0200″. Pl. LXXXII., Figs. 7 and 8. Navy Bay, Colon, Panama.

I have little hesitation in referring this magnificent form to the above-named species, the number of sides, like the number of nodules on *Eupodiscus* or *Aulacodiscus*, being of no specific value.

All forms of Triceratia with conspicuous hexagonal cells are, I believe, only varieties of *T. favus*. The radiating punctæ on the inner surface are not always present, and are really not on the inner surface of the valve; they indicate the presence of a thin silicious film, possibly the rudiment of a new valve; if a valve is crushed between the slide and cover, fragments of the film are detached.

In the first volume of the 'Lens,' a good Woodbury-type of *T. fimbriatum* (from a photograph of Dr. J. J. Woodward's) will be found, in which the punctæ are very distinctly shown. *T. fimbriatum* is rightly referred by Ralfs to *T. favus*.

Dr. M. Edwards ('Lens,' vol. ii., p. 105) mentions a six-sided form which agrees very well with my seven-angled variety, and which he calls *T. ponderosum*, but from the absence of any specific characters I am unable to decide upon their identity with certainty.* I have given the breadth of my largest specimen, the smallest I have seen measured about ·0120″; size is, however, of little or no value, even if this form had occurred in sufficient quan-

* His specimens were found in the Monterey deposit, in which he said a three-sided valve was also detected.

tity to have afforded means of estimating its variation; in the type form this variation is considerable. In a gathering from Sierra Leone I found a frustule of the type form scarcely exceeding ·0020″ from angle to angle.

Surirella contorta, n. sp., F. K.—Valve elliptically or slightly ovate, canaliculi fine, numerous, alæ inconspicuous, narrow median elevation terminating in short spines, surface of valve obscurely striate, valve in f. v. contorted. Sub-peat deposit, Mannawata, Wellington, and Wangarei, Auckland, New Zealand. Pl. LXXXI., Fig. 4. This fine species of Surirella is undoubtedly distinct from any form of this genus with which I am acquainted. The valves in the Wangarei are more robust than those in the Mannawata deposit, and of a yellowish brown colour, whilst those in the Mannawata material are hyaline, but differing in no other respect; the surface of the valve is blistered or puckered. The spines with which the elevated ends of the median space terminate form a very acute angle with the surface of the valve, and point in opposite directions.

I may here remark that the forms associated with the above are those usually found in sub-peat deposits, viz. Epithemia, Navicula, Himantidium, Cocconeis, Stauroneis, &c. *Navicula* (= *Pinnularia*) *cardinalis* is very fine in the Wangarei deposit, and differs from the typical form in the greater distance of the costæ and the broadly cuneate ends.

Stauroneis acuta is rare but fine in the above-named material.

III.—*Final Remarks on Immersed Apertures.*

By F. H. Wenham, V.P.R.M.S.

In closing this discussion I have to thank Col. Woodward for obligingly furnishing a diagram. When I requested this I did not expect the forthcoming tangible proof by direct measurement of the angles of the $\frac{1}{8}$th belonging to Mr. Crisp, made by Mr. Tolles; this having decided against the alleged extra aperture, the argument may also end with him. Some, however, still follow and uphold these ultra rays on theoretical grounds, and call for a notice which may be brief.

Mr. Keith's illustration in the September number of this Journal refers to a $\frac{1}{10}$th. I need not raise any question of its accuracy as a mere diagram, as I doubt the first position taken, and the direction of the rays that follow. I could give in diagram a dry lens admitting the largest pencil possible, that when immersed would fall within the theoretical limit, and so argument might be continued *ad infinitum*, and quite uselessly, as we have the object-glass referred to in this controversy to settle the point by measurement.

It will be seen in the diagram that while the back lenses may assimilate for a $\frac{1}{10}$th, the front is of small radius and diameter, magnifying sufficiently for a $\frac{1}{12}$ or $\frac{1}{15}$. This is very different in size from the "unfortunate" tenth previously sent *also* to prove the extra immersion rays. I am not prepared to allow the front to be correct in size or position with its radiant point "*assumed*" as stated.* If it were moved into the position necessary for a dry object the focus would fall on or within the front surface, and in any position there will be no air focus. If the diagram is made up of uncertain measurements, what is the use of it? Mr. Tolles, from whom all dimensions come, has repeatedly supplied diagrams to suit his theory. On page 14 of this Journal for July last he assumes that in all conditions of the combined lenses an angle of 60° can be obtained from the back systems. He there places the hemispherical front in this angle just where it fills it best, and at an exceedingly long distance from the middle. On the other hand, in Mr. Keith's illustration it is brought very close; a contrast indeed to the former case, where the increase of aperture is to be obtained by actually *separating* the lenses.†

Of course Messrs. Woodward and Keith are not responsible for data, but the former gentleman tells us it is "*a diagram accurately constructed in accordance with the computed results*": ‡ having, therefore, been drawn to suit the proposition, it may be dismissed.

Referring back to Mr. Keith's correspondence, I find he first

* 'M. M. J.,' Sept., 1874, p. 124. † Ibid., July, 1874, p. 14.
‡ Ibid., Sept., 1874, p. 126.

figures an arrangement for obtaining full apertures in balsam—the same in principle as that described and carried out by myself more than twenty years ago, and yet "wonders that I cannot see it." He next appears as an advocate of the correctness of Col. Woodward's diagram,* wherein he assumes his immersion radiant points close, and yet closer, in order to show that rays of any degree of obliquity can be got through a hemisphere, regardless of the destination of all of them to the focus at the eye-piece, thus carrying the argument round again to its commencement. On this Mr. Keith gives his verdict that Col. Woodward is right and I am wrong; and I may state, in reference to his last communication, that I do not think that it is possible to discriminate and decide in such a complicated optical arrangement as a microscope object-glass, that the spherical aberration is "practically nothing by computation" † and—on paper. It would be a great boon to the makers of object-glasses if this could be done.

It is still a matter of surprise to me that these gentlemen cannot see or will not admit that if an object-glass, whether made by Mr. Tolles or anyone else, be set at the immersion adjustment, and the angle considered as near Mr. Tolles' 180° as anyone may have the temerity to venture, till it occupies the whole of the front lens (for in dry lenses of large aperture the rays nearly fill the hemisphere), and with the focus close to the glass, that on dipping it into balsam, whatever the including aperture may have previously been, the cone immediately falls within 82° by the first law of refraction, the back focus remaining the same in both cases. But the bias of the discussion has been to show myself wrong by Mr. Tolles presumably getting some extra immersion rays, if only to the extent of 10°, 5°, yea, even half a degree. For the decision of a scientific fact, glasses besides Mr. Tolles' might be referred to in support, as immersions are made in other countries quite unsurpassed; but it is a triumph for him only.‡

Finally, a few words concerning the slit in focus of object-glass, for cutting off all these disputed or false rays. It is difficult to annihilate this by theory. Having given the death-blow to Mr. Tolles' extra apertures, it may be treated with but little notice, but cannot be got rid of as a thing of no account or a mere sensational affair. Col. Woodward says, "This method might be used without giving rise to material inaccuracy when the objective is adjusted for uncovered objects; but when it is closed to the point of maximum

* 'M. M. J.,' Nov., 1873, p. 212.

† Ibid., Sept., 1874, p. 124.

‡ "One maker having made a myth of the limit, it is probable that the rest will soon follow. But Mr. Tolles richly deserves high praise from all who use microscopes, *and all who make them*, for perseverance in the mechanical expression of his *correct perception* of the case, in opposition to high theoretical authority."—R. Keith, 'M. M. J.,' June, 1874.

aperture its spherical aberration is of course no longer corrected for uncovered objects." In measuring varying angles of aperture by the usual method, we take them at all points of the adjusting collar, and do not place in front a thickness of glass suitable for that correction, because with a parallel plate of glass there is no perceptible difference. The angle at the crossing point of the rays is the same whether it is there or not. I stipulate that the edges of the stop shall be in the crossing point. If anyone thinks proper to introduce an intervening plate of glass, serving no purpose, he must focus through it, so as still to get the stop in the focal plane. Further, if the collar is set for an object immersed in balsam, for the purpose of testing its reduced aperture therein by the means I have described, the slit must be set in focus, whether air, water, or balsam is the intermedium. In Mr. Tolles' $\frac{1}{6}$th the immersed aperture was found to be the *same* with all three, simply because they are parallel plates.

The question has now been so well ventilated that there is no use in wearying readers with further theories and counter-theories, perhaps only noticed by those engaged in the controversy, which few care now to read. Anyone free from prejudice, and recognizing the importance of cutting off all false rays within the focal point, would use a suitable stop for the purpose, and throw all controversial papers aside in favour of the practical proof in which the whole question must culminate at last. All this voluminous correspondence has arisen from a very small beginning, in which I pointed out an optical error of Mr. Tolles in the direction of rays, and which he unwisely chose to deny. In his last production of 180° he has gone ahead of all others; no one has surpassed him in that, and perhaps argument will not be wanting to prove by diagrams that he is right.

There I leave him, not without some amusement at the grotesque fatuity that induced his colleague to select the chaste and Christianlike motto, "A blunder is worse than a crime!"*

* See 'M. M. J.,' May, 1874, p. 228.

IV.—*The Filaria immitis. Amended Anatomical Details.*

By F. H. Welch, F.R.C.S., Assistant to Professor of Pathology, Army Medical School, Netley.

In my paper* descriptive of the thread-worm, *F. immitis*, the Canine Hæmatozoon, among other anatomical details of the parasite two conclusions are arrived at which subsequent dissections have proven to require modification; I refer (*a*) to the assumed cœcal termination of the intestine and consequent absence of anal aperture,† and (*b*) to the merging of the two uterine channels diverging from the vagina into one membranous tube continuous with the convoluted ovarian tubes at the tail end of the female worm.‡

Further examination of more recent specimens has shown me that these details do not correspond with the facts since brought to light, and as they are important points in the anatomy of the worm I hasten to correct them.

Termination of the Alimentary Canal.—Plate XXX., Fig. 2, female worm, and Plate XXXII., Fig. 16, male worm, illustrate a cœcal termination of the canal. However, on getting rid of the cutaneous envelope of the tail end of the parasite by maceration and dissection, and taking away most of the muscular parietes with the contained ovarian coils, I have traced the intestinal canal, contracted from the average $\frac{1}{400}$th inch to $\frac{1}{1000}$th inch in diameter, from beyond the assumed cœcal termination of the delicate tube to within $\frac{1}{100}$th inch of the terminal end of the worm, and when tension was brought to bear on this short unexplored portion the canal separated invariably from the inside of the muscular tip with a ruptured end and dimpling in of the external surface. The walls of the intestine are so thin that when collapsed it is easy to overlook the tube, which is mainly indicated by its dark contents; and when these stop abruptly, accompanied by a reduction in calibre of the canal, it is not difficult to be misled in favour of a cœcal termination. The presence of much dark granular material in the peri-visceral cavity at the tail end prevented the tracing of the delicate tube through it with absolute certainty, yet considering that up to within $\frac{1}{100}$th inch from the tail end it could be unquestionably followed, and that a ruptured end invariably ensued on tension being applied to this short obscured portion with indrawing of the outer surface, I think that a blind ending to the alimentary canal may be fairly negatived. Hence I conclude that an anal aperture is present in this blood parasite, and to make the sketches in accordance with these details it becomes necessary to extend the tubes, marked *a* and *p* respectively in Plate XXX., Fig. 2, and Plate XXXII., Fig. 16, to the extreme tip of the tail.

* 'Monthly Microscopical Journal,' Oct. 1st, 1873.
† Ibid., p. 160. ‡ Ibid., p. 161.

I will also add here that careful examination of the guinea-worm (*F. medinensis*) has led me to conclude that in this congener worm also the intestine terminates in an anal orifice a little within the concavity of the curled tail, and so in this anatomical point the worms are in unison.

Merging of the two Uterine Channels or Horns into one common Tube.—It is quite clear that this is incorrect. The two canals, diverging from the vagina, do not coalesce, but continue separate throughout, though placed in close apposition and bound one to the other by numerous fibres. When within a distance of the tail varying from 3 to $1\frac{1}{2}$ inches, each uterine horn merges into an ovarian tube, as detailed at p. 161 and illustrated in Plate XXXI., Fig. 7. The ovarian tube is coiled upon itself, but when extended averages 5 inches in length; it is continuous from one uterine horn to the other, and varies in diameter from $\frac{1}{200}$th inch at its junction with the uterine channels to $\frac{1}{100}$th inch midway, becoming again reduced to the smaller measurement at the extreme loop. The remarkably close binding of the one horn of the uterus to the other in their course from the vagina to the ovarian tube, and the difficulty of unravelling them without tearing their delicate walls and so obscuring accuracy of observation, led me to regard these tubes as one, but the tracing of the ovarian coils upwards has rendered clear these amended details of the generative system.

With these modifications I believe that the anatomy of the *Filaria immitis*, as detailed in my former paper, is substantially correct.

V.—*How to prepare Specimens of Diatomaceæ for Examination and Study by means of the Microscope.*

By A. Mead Edwards, M.D., Newark, New Jersey, U.S.

[The Author having kindly forwarded to us early proofs of the following article, we have much pleasure in giving it insertion.]

Having accumulated a number of gatherings of rough material, which, a cursory examination has shown, contain specimens of Diatomaceæ, and which, it is judged, it will answer to clean and otherwise arrange and put up, or, as it is technically termed, "mount," for future study, the intending diatomist requires to be informed how he may best set about preparing his specimens in the most advantageous manner. The author of the present sketch has published, in the seventh volume of the 'Proceedings of the Boston (Mass.) Society of Natural History,' certain directions for collecting, preparing, and mounting Diatomaceæ for the microscope; and as that paper contains a large part of the information he desires to

impart at the present time, he will draw upon it pretty freely, supplementing it to such a degree as later investigations warrant, or as may seem desirable.

Although most of the published treatises on the use of the microscope in general profess to give directions for mounting objects in such a manner as to preserve them for almost any length of time, and at the same time exhibit their characters to the best advantage, and although we have in the English language at least three books treating specially of this subject of the preparation of microscopic objects, yet hardly any one of these volumes gives any concise, practical, and at the same time reliable descriptions of the best methods of collecting, preparing, and mounting specimens of Diatomaceæ. In books, generally, when the preparation of these organisms is treated of, it is usually the fossil deposits which are considered, and even such directions as relate to these are for the most part meagre and unsatisfactory; and, when the specific and special directions are, as is often the case, copied from one book into the other without having been tested by the copyist, any faults they may have possessed, as originally written, are merely repeated and not eliminated. To prepare and mount specimens of Diatomaceæ, for the purpose of sale alone, is one thing, and to prepare and mount them, so as to preserve and exhibit their natural characters and fit them as objects of scientific study, is another and very different thing. The latter can only be attained after considerable practice, and to do it properly a considerable amount of knowledge of their natural history is plainly necessary.

The Diatomaceæ should always be prepared and put up for a special purpose,—that of exhibiting characters peculiar to genera and species; and to do this those characters must of course be known. Muds, guanos, dredgings, and gatherings of that description can seldom be used for the purpose of exhibiting such characters, and when they can, in exceptional cases, be so employed, it is when the forms they contain are selected out in the manner to be described hereafter. Gatherings, likewise, which contain many species in a mixed condition, should, as a general thing, be rejected, unless there be present something of special importance, such as rare species, or some large and fine or distorted forms of common species. But even in such cases it will be found best not to mount the gatherings as collected, but to select out the forms desired and place them upon slides by themselves, and in such media as will exhibit their peculiarities to the best advantage. Of course it may be desirable to study the geographical distribution of the Diatomaceæ; and then mixed gatherings become of value as exhibiting the number of forms occurring at a particular station. Then, again, the fossil as well as the semi-fossil deposits and guanos may be cleaned and mounted as obtained; but even then it may become

desirable, if space can be spared in the cabinet, to have the various species found in each gathering separately mounted, so that they may be at any time studied in comparison with similar forms from other localities.

General directions for collecting Diatomaceæ have been already given in part seventh; but it will be desirable to again allude to a few points in connection with this portion of our subject. Some years since, an article entitled "Hunting for Diatoms" was published in a London journal called 'The Intellectual Observer.' The author's name was not given, but internal evidence would seem to indicate that it was penned by a deceased botanist of note, who was a decided authority on this branch of biology. This paper contains some valuable hints respecting the places in which to look for diatoms, and some of the suggestions contained therein I have ventured to transfer to these pages, as they will be found of value to the intending diatomist. Thus, the exquisite *Arachnoidiscus, Triceratium Wilksii*, and *Aulacodiscus Oregonensis,* may be looked for on logs of wood which have been floating in the sea, and imported from New Zealand, or Vancouver's Island. So, on logs from Mexico and Honduras may be found the curious *Terpsinæ musica.* The nets of fishermen, especially from deep water, may yield algæ bearing such forms as *Rhabdonema arcuatum* or *Adriätium, Grammatophora serpentina* and *marina,* various *Synedras,* and other fine forms. On oyster shells may be found algæ bearing upon their fronds *Biddulphia regina, Baileyii* or *aurita. Rhizosolenia styliformis* is said to be almost sure to be there likewise. After a ship is unloaded, and as it floats higher in the water, its sides may be searched for treasures of the diatom world, and *Achnanthes longipes* and *brevipes* found, or even *Diatoma hyalinum* and *Hyalosira delicatula.* The sea-grass, or *Zostera marina,* growing along our coast, often bears upon its waving ribbons fine forms of diatoms, and that used for stuffing chairs, and lounges or mattresses, and imported from abroad, will yield foreign species to the collector. There is a plant known in England as "Dutch rushes," which is imported into that country from Holland, and which is used for chair bottoms. These plants grow in the brackish water of the marshes, and hence upon them are to be found the delicate *Coscinodiscus subtilis, Eupodiscus Argus,* and *Triceratium favus.* Both of these two last-named forms occur commonly on our Atlantic coast, and muds from Charleston, S.C., and Wilmington, Ga., have provided me with them in plenty. Cargoes of bones, which present green incrustations from having lain in the water for some time, are said to yield diatoms, some of which may be rare, as coming from foreign ports. The state of New Hampshire has not yet been sufficiently gone over for it to be said what the characteristic forms of Diatomaceæ growing within its boundaries are, but yet we

may safely predict that the lakes, ponds, streams, and sea-coast of that state will yield to the searcher ample material of beautiful forms.

If the microscopist wishes to mount a few slides of recent diatoms just to show what diatoms are, nothing is easier. It is only necessary to boil a small mass of them in strong nitric acid in a test-tube over a spirit lamp, and, when the acid has ceased to emit red or yellowish fumes, wash them thoroughly with clean water, allowing them to settle completely. Then a little of the clean sediment, consisting almost entirely of the shells of the diatoms, is taken up by means of a "dip-tube," and placed upon the central portion of a glass slide. Here it is dried, and the slide warmed over a lamp; then a drop of Canada balsam is permitted to fall upon the diatoms. As soon as all bubbles have cleared off from the balsam, a warm cover of thin glass is carefully laid upon it and permitted to settle into place. When cool, it is ready for examination by means of the microscope, any balsam which has exuded around the cover being washed off with alcohol. In this way rough and tolerably clean specimens may be obtained; but such would not, or, at all events, should not, satisfy the student of the Diatomaceæ. For him more elaborate methods are necessary, and these we will now proceed to consider.

Apparatus and Chemicals necessary.—A chemist's retort-stand, which is a heavy iron plate with an upright rod projecting from one side of it. Running on this rod, and so arranged that they may be fixed by set-screws at any height, are a series of rings of various diameters, which are to be used to hold the vessels in which the specimens are to be manipulated over the source of heat used. Mr. C. G. Bush, late of Boston, Mass., who has had considerable experience in cleaning Diatomaceæ, tells me that he uses a lamp burning petroleum oil, as cheaper than a spirit lamp, and, to support the vessels he employs, has a little metal arrangement on the top of the chimney, such as is supplied for the purpose of holding a small tea-kettle and the like. The only objection to the oil lamp is, that, unless the wick be well turned down, we are liable to have our vessels blackened. However, the heat given off by burning petroleum is very great, and I have often used such a lamp with advantage. If desired, of course, the source of heat used may be gas, burned in a Bunsen's burner, or a spirit lamp; and this last, especially if it be supplied with a metal chimney to cut off draughts, is, all things considered, the best, as it is very cleanly, not being liable to smoke the bottom of the glass or porcelain vessels used. If we are going to work with large quantities of material, we shall require a small sand-bath to heat the glass vessels upon. In small quantities, the diatoms may be boiled in test-tubes, when some sort of holder will be required. The metal ones, sold by

dealers in chemists' apparatus, are extremely handy; but I have found that we can make very good ones out of old paper collars. One of the kind called "cloth-lined" may be cut into strips about three-quarters of an inch wide and three inches long. Such a strip is folded around the test-tube near the top, and the ends, brought together, are held between the forefinger and thumb. In this way the tube is firmly grasped, and can be held over the lamp without much danger of burning the hand, as the paper collar strip is a bad conductor of heat; or the paper strip may be grasped in an "American clothes-peg," which has a spring to force its parts together. Large quantities of diatoms are best boiled in porcelain evaporating dishes, glass flasks, or beaker glasses. The last-mentioned vessels are also by far the best things for washing them in. A few, say three or four, glass stirring-rods will be found useful; and one or two American clothes-pegs to take hold of hot evaporating dishes with. Then there will be required a few dip-tubes, made of small glass tube, drawn out over a flame, so that the opening is considerably diminished. The mode of making these cannot be given here, but will be found in books on chemical manipulation; and it will be well for the student to learn to make his own dip-tubes, as a number will be required first and last, and they are easily broken. Of course there will be required a number of glass slides of the usual dimensions of three inches by one. These should be of as white glass as possible, and it will be found best to procure those with ground edges, as they are the neatest in appearance. Only such as are free from scratches or other blemishes in the central square inch should be used; and although even such as have bubbles or scratches near the ends only will not look ornamental in a cabinet, we should remember that microscopic objects are not generally mounted to look well in a cabinet, but to be useful out of it; so that if the central and useful portion of the slide be perfect it need not be rejected. Some persons make their own glass slides, but I have never found it answer to do so, as it is difficult to get the right kind of glass, not at all easy to cut it or grind the edges, and it is liable to be scratched while cutting or grinding. Thin glass, such as is made on purpose for microscopic use, will be required; and this, also, it will be found best to buy ready cut than attempt to cut it for one's self. The thin glass used for covers may be of different thicknesses, but the thickest made will not do for diatoms, and a certain amount of the very thinnest will be required for small and delicately marked forms, on which very high power objectives will have to be used. The covers must be perfectly clean, which may be ensured by soaking in caustic potassa solution, and then washing thoroughly in clean water. The thinner kinds of glass are rather difficult to clean; but with a little extra caution it may be accomplished, the last polish being given to it by a piece of an old and

well-worn cambric handkerchief. The covers, always round, should be separated into sizes and thicknesses, so that the exact kind of cover required can be found without having to search for it by turning over a number, scratching or breaking them, and losing much valuable time. We shall also require a pair of forceps for holding the slides over the lamp; and such as are sold at house-furnishing stores and by grocers, under the name of American clothes-pegs, and which have been already mentioned, are by far the best I have ever seen or heard of. A small pair of brass forceps which close with a spring will be needed, and they are best set in a wooden handle so as to protect the fingers from the heat; and another pair, which spring open and may be closed by means of the finger and thumb, will be wanted for taking hold of and adjusting the thin covers. I do not advocate the use of paper covers for slides, but labels of some kind will, of course, be required, and I have found the plain circular white ones to look the best. There are very pretty square labels sold by dealers in these things that I have used and liked. For making cells to hold specimens put up in a fluid, a turn-table and brushes and some cement will be necessary. The cement I use and prefer above all others is good old gold size used warm.

The chemicals required are nitric acid, sulphuric acid, hydrochloric acid, bichromate of potash, caustic potash, alcohol, and, above all, a plentiful supply of clean, *filtered* water. The water should be such as leaves hardly any residuum when a quart of it is evaporated to dryness; and it must be filtered just before use, to remove any minute organisms, diatoms especially, which it may contain. A certain amount of washing soda will be wanted, if guanos are to be cleaned.

We will now proceed to consider the manipulations necessary to prepare the various kinds of gatherings, always remembering that these methods will have to be modified to a certain extent for each specimen.

Recent Gatherings.—If there be sand in the gathering, it will be well to remove it before using acid, by shaking it in clean water and pouring off before the diatoms, which are lighter than the sand, settle. The water holding the diatoms in suspension may be poured into a test-tube, or beaker, the diatoms allowed to settle, and as much of the water poured off as possible. The diatoms are now covered with nitric acid to about the height of half an inch, and allowed to stand for a few minutes. Usually some chemical action takes place, and it will be well to wait until it subsides. The test-tube or beaker is then held over the lamp and carefully heated until the reaction of the acid upon the organic matter of the diatoms ceases. Thereafter, and while the liquid is still hot, I have found it often advantageous to drop in one or two fragments of

bichromate of potash. The organic matter is more thoroughly destroyed in this way than when the acid is used alone. Thereafter it is well to pour the acid and diatoms into a capacious beaker of clean water, washing the tube or smaller beaker out with a little water, and adding this to the other. After the diatoms have all settled, which will often require hours, the supernatant fluid is carefully poured off, and a fresh supply added; and this must be repeated several times until all of the acid and coloured chromium compound has been removed. When this point is arrived at can only be ascertained from experience. In this way the valves and connecting membranes of the diatoms are usually separated and cleaned ready for mounting, which process will be described hereafter.

Muds will have to be treated in a somewhat different manner from recent gatherings. If the mud is dry, it will have to be broken down by boiling for a few minutes in a solution of caustic potassa, the strength of which must be apportioned to the particular specimen under treatment. After it has been broken down into a soft mud, all of the potash is thoroughly washed off by means of clean water, and replaced by nitric acid, as in the case of recent gatherings. This is boiled, and a little bichromate of potash added as before, and the whole washed. It very seldom happens that the diatoms occurring in mud will be sufficiently cleaned by this process, so that it has to be supplemented by another. The sediment is therefore washed into one of the evaporating dishes and allowed to settle, and as much of the water poured off as possible. Then sulphuric acid, in quantity to a little more than cover them, is poured in, and the vessel gradually and carefully heated. As soon as the liquid shows signs of boiling, bichromate of potash is added, a very little at a time, until the green colour first formed by its reaction upon the organic matter begins to assume a yellowish tint, when no more is dropped in; but a few drops of hydrochloric acid are permitted to fall in, and the liquid is allowed to cool. Of course it will be best if the person undertaking to clean diatoms is somewhat versed in the use of chemicals; but at any rate care must be taken not to drop any of the acids upon the clothes or skin, and great caution must be exercised in not inhaling any of the vapours given off. Those evolved after the addition of the hydrochloric acid are especially irritating and dangerous, and must be avoided. As soon as the liquid has cooled a little, water should be added cautiously, as great heat will be generated thereby, and there will be danger of its boiling over. Thereafter it may be poured into a large beaker-glass of water and thoroughly washed as in the former case. If it be found that the precipitate is not quite white, it will be necessary to boil it again in sulphuric acid, with bichromate of potash and hydrochloric acid, until it is quite clean. If, on examination by means of the microscope, it is found that there is much

flocculent matter present besides the diatoms and sand, this can be removed by boiling for a few seconds in a weak solution of caustic potash, and washing quickly and thoroughly with plenty of clean water. When we have recent gatherings of filamentous or stipitate forms of Diatomaceæ, which we desire to preserve in the natural condition, they should be immersed for about twenty-four hours in alcohol to dissolve out the endochrome. If this does not answer, it will be well to soak the mass of diatoms or plants upon which they are adherent in a solution of hypochlorite of soda, an impure variety of which is sold in the shops under the name of Labarraque's disinfectant, for about the same length of time. This will generally destroy all colour, and leave the specimens transparent. It is best, however, in many cases not to remove the endochrome, but leave it, and mount the specimens in such a way as to show them in as natural a condition as possible. How this may be done will be described hereafter.

Guanos.—The preparation of these substances, so as to obtain the microscopic organisms they may contain, is rather difficult, tedious, and dirty, and should only be undertaken by a person somewhat versed in chemical manipulations, and in a proper room as a laboratory, where there is no danger of harm resulting from the fumes evolved. As the ammoniacal guanos are those which contain the most diatoms, and consequently which answer best to clean, we will begin with them, and take as a type that which comes from the islands on the coast of Peru. As it comes into commerce this guano is a moist powder of a light iron-rust colour, smelling strongly of ammonia, and having scattered throughout its mass lumps of ammoniacal salts of a more or less solid consistency. The guano should be thinly spread out upon a stiff piece of paper and exposed to the air, and, preferably, to a moderate heat for several days or even weeks. In this way most of the moisture and much of the ammonia will evaporate, and less acid will be required to clean the guano. It will now have become much lighter in colour, and crumble to a dry powder. A tin pan is now about half filled with a solution of common washing soda in clean filtered water, and placed over some source of heat, as on a stove. The strength of this solution is not a matter of any great moment, and must vary with the guano manipulated. As soon as it begins to boil, the guano is dropped gradually in, a little at a time, while the liquid is stirred with a glass rod or stick of wood. Considerable effervescence takes place, ammonia being given off, and therefore it must be kept continually stirred, and care exercised to prevent its boiling over. After a while it is poured into a plentiful supply of clean water and washed therewith several times, care being taken to permit all of the diatoms to settle. As soon as the wash-water is only slightly coloured, the guano is transferred to a good-sized evaporating dish,

and covered with nitric acid, and boiled. While it is boiling, a few crystals of bichromate of potash are dropped in, and the material washed as in the case of muds. Thereafter the diatoms are boiled in sulphuric acid with bichromate of potash and hydrochloric acid, as before described.

Phosphatic guanos, as that from Brazil, are somewhat more difficult to treat. They are generally drier than the ammoniacal kind, and must be boiled in a large quantity of hydrochloric acid as many as three times, and the acid must be poured off while still hot. Thereafter nitric acid and sulphuric acid and bichromate of potash must be employed, as in the other case.

Lacustrine Sedimentary Deposits.—For the most part these are pulverulent, and easy to clean. Some, as found in nature, are so pure that they require no cleaning except washing in clean water. Burning on a plate of platinum or mica will often serve to clean some specimens; but it will, in general, be found best to boil in nitric acid with a little bichromate of potash, and subsequently in sulphuric acid and bichromate of potash, with the after addition of hydrochloric acid. Occasionally a certain amount of flocculent matter will be left, which it will be necessary to remove with very careful heating, not boiling, in a weak solution of caustic potash, and immediately pouring into a large quantity of clean water and thoroughly washing.

Marine Fossil and Sub-Plutonic Deposits, being stony, and possessed of very much the same physical characters, are manipulated in the same manner. A small lump of the deposit is placed in a test-tube, and covered with a strong solution of caustic potash. It is then boiled for a few minutes, and usually it immediately begins to break up and fall down in the shape of a soft mud-like material. At once the liquid, with the suspended fine powder, is poured off into a large quantity of clean hot water, and if the whole of the lump has not broken down into a powder, what remains has a little water poured over it in the test-tube, and it is again boiled. It will be found that a little more will now crumble off. This is added to the rest in the large vessel, and if the lump has not now broken down, it is again boiled in the alkaline solution and in water alternately, until it has all been disintegrated. It is then all permitted to settle for at least three hours, when it is thoroughly washed and boiled in hydrochloric acid for about half an hour. There is then added an equal amount of nitric acid, and the boiling continued for a short time. It is then washed and heated in sulphuric acid, with the addition of bichromate of potash and hydrochloric acid.

All mixed gatherings of Diatomaceæ, and particularly all muds and deposits, should be separated into densities, so that for the most part the larger forms are collected together, free from sand, and separate from the smaller species and broken specimens. This is

done by using a number of beaker glasses, of various sizes, in the following manner:—Into a 1-ounce beaker the cleaned diatoms are placed, and the vessel filled with water. It is then well stirred up by means of a glass rod, and, after resting about five seconds, poured off carefully into a 6-ounce vessel so as not to disturb the sand which has settled. Again the vessel is filled up with water, stirred, allowed to settle for the same length of time, and poured into the same vessel. This is repeated until it has been done at least six times, when we shall find all of the sand, free from diatoms, in the small beaker. This can be thrown away, and as soon as the material in the large beaker has settled it is returned to the small one, and the same process gone through with, only extending the time of settling now to about ten seconds. The next density is that which settles in twenty seconds; and so on, five or six densities may be obtained, and if carefully prepared they will be found to contain forms varying very much one from the other. The large species of *Triceratium*, *Aulacodiscus*, and the like, will be found in the coarsest density, and the broken diatoms in the lightest.

Preserving and Mounting Specimens so as to have them in a condition for study at any future time.—Of course, when possible, Diatomaceæ should be studied in the living condition. But there are many forms which have not been as yet found living, and these can only be studied as dead skeletons; and, in fact, it is in the dead skeletons of the Diatomaceæ that many of the most marked characteristics are to be found; and on such characteristics species have been founded. Besides, the most beautiful sculpturing of the valves is only to be seen after everything has been removed but the silicious cell-wall I have termed the skeleton. Therefore I advocate the cleaning of a portion at least of every gathering in the manner described, so that nothing will be left but the clean silicious cell-wall.

If we desire to keep specimens in a state as near that they present when living as possible, we have to put them in some preservative fluid in which they will not decay, and in which the softer parts will be preserved. Unfortunately these soft parts do not keep well; but the fluid which I have found to be the best for the purpose is distilled water, which has to every fluid ounce two or three drops of wood creosote added, and thereafter a sufficient number of drops of alcohol, which will be about double the number of the drops of creosote, to make the creosote soluble in the water, which it is only to a very slight degree under ordinary conditions. I do not advocate any fluid containing glycerine, or, in fact, any of the preservative fluids described in the books treating of the preparation of microscopic objects. The vessels in which the fresh specimens of Diatomaceæ are put up are what are known to microscopists as "cells," but how these are made cannot be gone into here, as the description would occupy too much space and time.

Suffice it to say that I prefer cells made of old japan gold-size, which can be procured of dealers in microscopic materials. Within such a cell, of sufficient depth and immersed in the preservative fluid, a few of the diatoms, or a scrap of the plant upon which they are growing, is placed, and the glass cover fixed over it in the manner described in the books upon manipulation. The filamentous forms are thus preserved almost in their natural condition; but, on account of the presence of the endochrome, the sculpturing of the silicious cell-wall is almost invisible. To show this character, while the filamentous form is preserved, another method of mounting is employed. A thin, clean covering glass is selected, and laid upon a clean piece of paper. A large drop of distilled water is then allowed to fall upon it, and in this drop the filamentous diatom is thinly spread out. Then the cover is taken up by means of a pair of forceps and held over the flame of a spirit lamp, which has been turned down so as to be quite small and steady. The cover is held some distance above the flame, and judiciously manipulated, so that the heat is evenly distributed over it, and it does not crack. As soon as all the water has been driven off without the formation of bubbles, the glass is brought gradually down almost in contact with the flame, and held at that point for a few minutes. Then the diatoms will be seen to turn black, on account of the charring of the organic matter contained in them. After a while this black carbonaceous matter will burn off, and they will become quite white. If, however, there seems to be any difficulty in burning off the last portion of carbon, the cover is lowered once or twice to come in contact with the top of the flame, and then raised again. In this way it will become red hot for a moment; and everything will be burned off except the silicious portions of the diatoms. Now the cover is removed slowly from over the flame, and held in the forceps until it is cold, but by no means laid down upon any surface until it is quite cold, otherwise it will fly into pieces. Then it can be laid upon an ordinary glass slide, and examined to see if it is worth preserving, which may be done in one or two ways: first, the glass cover is warmed, and a drop of good spirits of turpentine let fall upon it, covering the diatoms. Just before the spirits evaporate, a small drop of thin Canada balsam is added, and a slide taken, warmed, and a drop of balsam placed upon the centre part of it. Then the cover is brought down upon the slide, the two balsam-covered sides together, in such a way, by tilting the cover slightly, that no air is allowed to come between them, and the cover permitted to fall gradually into place, driving a wave of balsam before it. In this way we have the filamentous diatoms arranged as they grow, but with endochrome removed which would obscure the markings, and in balsam, which renders them transparent. Some forms, as some of the

Fragillariæ, become too transparent if put up in this way, and therefore another method of mounting must be adopted with them. They are burned upon the cover, as just described, but mounted dry in air; that is to say, a cell of gold size is made, the glass cover slightly warmed, and then placed upon the cell, with the side upon which the diatoms are fixed, downwards. The warmth slightly softens the gold size, and the cover becomes fixed.

Other forms besides the filamentous species may be mounted in fluid, or burned upon the cover and subsequently put up in balsam, or dry. But the commonest way of treating such forms is to clean them by means of chemicals, as already described, and then, previous to mounting them, divide the clean gathering, consisting of a white sediment of large and small diatoms along with fine sand, all mixed up together into densities. Of course, if some of this sediment were to be mounted in this condition, extremely unsightly slides would be procured; so it is best to separate the finer from the coarser diatoms, and these in turn from the sand. This is accomplished by what is known as elutriation, or, separating into densities after the manner already described. Then slides may be mounted from each of the densities in the following manner. A slide is thoroughly cleaned, and a good-sized drop of water placed upon the centre portion. A little of the diatom sediment is then taken up in a dip-tube, and the point of the tube brought just into contact with the drop. As soon as a few diatoms have run out of the dip-tube, it is removed. Then a small splinter of wood or stiff bristle is used to disseminate the diatoms through the drop of water in such a way that they will be pretty evenly distributed and not overlie each other. The water is then driven off by heat, a drop of thin Canada balsam placed upon the dry diatoms, and a cover placed on them in the usual manner. In many cases, especially when dealing with the smaller forms, it will be found desirable to mount them upon the cover in this same way, instead of upon the slide, as they will then be brought as near as possible to the objective of the microscope. Single or remarkable specimens of diatoms may be picked out and mounted by themselves; but the manner of accomplishing this would occupy more space than it has been thought desirable to devote to this portion of our subject, and the reader is referred to the books on mounting microscopic objects for the particulars of the process.

The main principles of preparing and mounting Diatomaceæ for preservation and study have been given, and the intending student will be able to devise modifications and improvements for himself, so that he will be able to put up specimens in as finished a manner as any to be procured from the dealers.—*From the Report of the Geological Survey of New Hampshire*, vol. i.

NEW BOOKS, WITH SHORT NOTICES.

The Anatomy of the Lymphatic System. By E. Klein, M.D., Assistant Professor at the Laboratory of the Brown Institution, London. I. The Serous Membranes. London: Smith, Elder, and Co., 1873.—Some of the results of the observations described in this memoir were published in 1872, in the January numbers of the 'Centralblatt für die Medicinischen Wissenschaften.' Dr. Klein has since that time carried his observations farther, and is of opinion that the anatomy of the serous membranes, so far at least as their lymphatic system is concerned, may be regarded as complete. The present work deals with the minute structure of the omentum, the centrum tendineum of the diaphragm, and the mediastinal pleura, both in their normal and pathological conditions.

It is almost impossible to criticize a book of this nature. The Teutonic carefulness, which insists on a knowledge of all contemporary work on a given subject, precludes almost any farther inquiry into facts and arguments adduced on one side or the other, where the only possible proof is a repetition of the writer's work and experiments. And it is only the careful histologist who can verify or pull to pieces such work as Dr. Klein's. Mere bibliographical research, with the addition of one or more presumably new facts, not necessarily supporting, or even antagonistic to, some stated text, is certainly too common a characteristic of authors in German "Archive" and "Zeitschriften." But no such fault is visible here. The facts observed are stated plainly and severely; the opinions of others put forward with fairness, whether they agree or differ with those of the writer; the conclusions of the observer himself offered modestly, though with no lack of firmness.

In his first chapter he points out that on the serous membranes in certain regions there is normally to be seen a *germinating* endothelium. For instance, the fenestrated portion of the omentum in various animals shows numbers of cells which are raised from the general surface by means of a stalk, and which possess in their peripheral spherical portion two nuclei or a nucleus in a state of division; and besides the appearance of constriction, and division of these polyhedral or club-shaped endothelial cells, there are always numbers of smaller spherical lymphoid elements which are detached from the surface, that is to say, which have become perfectly separated. The same character is possessed by the endothelium of the surface of certain nodular or cord-like structures, which are either isolated or in connection with the chief trabeculæ of the fenestrated part of the omentum, in which larger blood-vessels or fat are contained. In the second chapter, in which he discusses the cellular elements of the ground-substance (matrix, basis-substance), he shows that the knots and cords are not exactly to be regarded as pre-existing adenoid tissue, nor as collections of lymph-corpuscles, but that they are developed for the most part out of the ordinary branched cells of the

tissue—in fact, as "peri-lymphangial nodules and tracts;" and that the farther the development is advanced the more numerous are the lymph-corpuscles at the spot—the more does the cellular network assume the character of an adenoid network. From their topographical arrangement they are analogues of the fat tissue of the omentum; and the writer considers at length their conversion into fat nodules and tracts. In the third chapter he describes the lymphatic vessels of the serous membranes, their relation to the surface of the latter, and the development of lymphatic capillaries. He distinguishes two kinds of stomata on the surface of the serous membranes, stomata vera and stomata spuria or pseudo-stomata, the former representing either the mouth of a vertical lymphatic channel, or a discontinuity of the endothelium of the surface. He points out also that normally in the omentum and the mediastinal pleura, knots and cords originate by the outgrowth of the endothelium of the lymphatic capillaries as a network of branched cells from which young cells spring. The fourth chapter is taken up with the distribution of blood-vessels in the lymphangial nodules and cords, and the development of blood capillaries. The principle according to which the latter takes place he finds to be similar to that which was first pointed out by Stricker in the new formation of blood-vessels in the tadpole and in inflammation, and confirmed in every particular by Arnold afterwards. In a patch in which there is a considerable number of young vessels one capillary may easily be found which ends cœcally. At the cœcal end the protoplasmic character of the wall may be recognized very clearly; the lumen becomes more or less suddenly narrowed, the wall finally becomes solid, and passes into a perfectly ordinary nucleated branched cell of the matrix. In the second section he sketches briefly the views of different writers on inflammation and artificial tuberculosis, and shows that they are all, more or less, borne out by his own description of the normal serous membranes. For instance, Ranvier and Kundrat confirm the germination of the endothelium in inflammation of these tissues, and the latter considers the miliary nodules on the serous membranes in tuberculosis to be derived from the same source. Sanderson holds that there is a pre-existing adenoid tissue in the omentum, in the form of nodules and cords, which increases to an extraordinary extent in chronic inflammation, so that the nodules and cords of tubercle found in artificial tuberculosis are nothing but hyperplastic adenoid tissue. Knauff's views on the same subject are precisely similar; while Klebs declares that in the same affection the lymphatic vessels play a most important part, their endothelium proliferating, and thus composing the tubercle-knot. The book closes with the observations of the author himself on the changes found in these serous membranes in acute and chronic peritonitis; and in an interesting, though somewhat abrupt tailpiece to a most interesting volume, a fact is mentioned which has bearings somewhat different to the intended scope of the book, the occurrence, namely, of the ova of certain entozoa in the lymphatics of the omentum and mesentery of rabbits and cats.

For reasons stated at the commencement of this article, we have

purposely contented ourselves with abstracting, as far as possible in the words of the author, the matter of this work. The Government Grant Committee of the Royal Society are able to congratulate themselves on having furnished the means for the execution of the numerous plates, which to our eyes are better, as being less hard, than the engravings attached to 'The Handbook for the Physiological Laboratory.' It seems almost a pity—though it may be cavilling about a minor matter—that so handsome a volume as that under notice should be somewhat spoiled by certain faults of idiomatic style, which no one, we are sure, would more willingly have overcome than the writer himself. To the same absence of careful supervision on the part of a friend are probably due one or two obvious misprints, perhaps scarcely worth mentioning. But the work adds one more to the numerous causes of self-congratulation with which the English scientific world rejoices over the connection of Dr. Klein with the Brown Institution.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Egg-peduncles of certain Worms.—Professor Moebius figures and describes a new genus of chætopod worms with external ovaries from the eighteenth segment onwards; they are situated below the branchiæ, and at the boundary between the two segments. Within the body-wall in the same segments are also eggs. The worm is named *Leipoceras uviferum.* Moebius has discovered that another worm (*Scolecolepis cirrata* Sars) carries its eggs in pouches like a swallow's nest, along the hinder segments of the body. Many Polychætous worms bear their eggs in sacs attached to the ventral surface of the body (e. g. *Autolycus prolifer* Müll.). One (*Syllis pulligera* Krohn) carries them in the shorter dorsal filaments of its feet, while in *Spirorbis spirillum* the eggs are carried in folds of the skin, developed in the peduncle of the operculum, with which it closes its tube.

Tornaria, the young of a Worm.—It is stated in the 'American Naturalist' for July, that Mr. Alexander Agassiz has discovered that the Tornaria, an immature microscopic floating animal, which he in common with other naturalists had thought to be a young starfish, is really a young worm. The parent is a remarkable worm, found at different points on the American coast and that of Europe, burrowing in sand, and described by the celebrated Italian zoologist Delle Chiaje. The history of Balanoglossus as given by Agassiz "while showing great analogy between the development of Echinoderms and the Nemertian worms, by no means proves the identity of type of the Echinoderms and Annuloids. It is undoubtedly the strongest case known which could be taken to prove their identity. But when we come carefully to analyze the anatomy of true Echinoderm larvæ, and compare it with that of Tornaria, we find that we leave as wide a gulf as ever between

the structure of the Echinoderms and that of the Annuloids." Now the young of certain Echinoderms have a form very similar to larval worms. "On this chiefly Professor Huxley, misled by the names given by J. Müller to some of these larvæ, has revived the old opinion of Oken, and associated the Echinoderms with the Articulates; but as he based his opinion entirely upon the figures of Müller, and not upon original investigations, his conclusions, which have been adopted by the majority of English naturalists, do not appear to Mr. Agassiz as tenable. The hypothetical form to which Huxley reduces these larvæ, to make his comparisons and to draw his inferences, is one which has never been observed, and as far as we now know does not exist." Mr. Agassiz's paper, with many beautiful figures, appears in the 'Memoirs of the American Academy of Arts and Sciences.'

The Structure of the Cornea.—This has been very well investigated by Dr. Thin, who has communicated a paper on the subject, through Professor Huxley, to the Royal Society. The paper appears in the last number but one of the 'Proceedings of the Royal Society.' The author says, referring to his former paper,* that in "order to corroborate the results yielded me by the nitrate of silver, I availed myself of the well-known property which hæmatoxylin possesses of specially staining the nuclei of cells. I allow the cornea to remain in the solution until it is perfectly saturated. Subsequent maceration in acetic acid removes the hæmatoxylin from the fibrillary substance before it bleaches the nuclei. On comparing a cornea so treated with successful preparations of the cornea-corpuscles as obtained by chloride of gold, it is found that the number of cells demonstrated by the hæmatoxylin exceeds by several times that found in the gold preparation, affording direct proof of the existence of other cells in the cornea than those known as the cornea-corpuscles. If a vertical section of the cornea is so treated by hæmatoxylin and acetic acid, in many of the clefts of the fibrillary substance, in which, as is well known, the cornea-corpuscles are situated, several nuclei are seen, proving in another way the existence of a greater number of cells than those hitherto accepted by anatomists. But in addition to the proof afforded by staining the nuclei of the cells, I have, by the application of a new method, been able to isolate (and thus demonstrate beyond all further possibility of doubt their existence in the cornea) a large number of cellular elements, the varied size and shape of which distinguish them not only from the cornea-corpuscles, but from any anatomical structures that have been as yet described. If a cornea is placed in a saturated solution of caustic potash, at a temperature between 105° and 115° Fahrenheit, it is reduced in a few minutes to a white granulated mass of about a fourth of its previous bulk. In a small piece of the diminished cornea, broken down with a needle and examined under the microscope in the same fluid, it is found that the only visible elements are a great number of cells. If the conjunctival epithelium of the cornea has not been previously removed, the cells of that structure can be recognized amongst the others; and if the mass under examination has not been too much broken up in manipulating,

* 'Lancet,' February 14.

groups of them may be seen in direct anatomical continuity with long narrow flat cells, which belong to the elements that have been for the first time brought to light by the potash solution. But the cells of the anterior or surface epithelium form a very small proportion of the number. The smallest piece that can be removed by the needle from a cornea which, before being put into the solution, has had this epithelium scraped off and Descemet's membrane removed, shows under the microscope a multitude of cells. Of the branched corpuscles, the fibrillary substance, and nerves, not a trace is visible. The form of these cells is so various that it would be difficult to construct a series of types under which every individual cell could be brought. They seem in their development to have assumed any modification of form that is necessary to enable them to fit accurately the cavities and fibrillary bundles to which they are applied. Those whose outlines do not permit their being accurately described as belonging to a strictly defined type, are many of them somewhat quadrangular or triangular in form, or club-shaped, with a short or long projecting process. Of fixed and definite types are long narrow rods, ending obliquely at the point, and oblong cells intersected at one end by a notch, which receives the extremities of two of the long cells that lie parallel to each other. I do not attempt to give an exhaustive account of the various forms assumed by these cells. A better idea than can be given by any description will be got by an examination of figs. 1, 2, 3, plate viii., in which many of them are represented; but an examination of the first-prepared cornea will show that there are many forms and modifications which have not been drawn. The cells are granular in appearance, with sharp clear outlines. The terminal surfaces of the long cells can often be seen to be finely serrated; and so closely do they fit each other at these points, that sometimes a high magnifying power is necessary to discover the suture-like line by which the junction is indicated. The nuclei of all the cells have nearly the same length, but in the narrower cells the nucleus is often much compressed transversely. The long cells are many of them 0·09 millim. long, and from 0·006–0·003 millim. broad; the shorter cells are broader. Those 0·06 millim. long are generally about 0·009 millim. broad. A length of 0·36 millim. with a breadth of about 0·015 millim. is common; others are 0·03 millim. long and 0·012 millim. broad. I have chiefly examined the cells in the cornea of the ox, sheep, and frog, and have found no important differences either in shape or arrangement. In examining portions of the cornea which have been as little disturbed as is consistent with the maintenance of transparency, groups of cells are found massed together *in situ*, as they have been left by the dissolving out of the fibrillary substance by the potash: these are found chiefly in two forms. Transverse masses of the anterior epithelium are found sutured to long narrow cells, which sometimes seem to join them at an angle. Further, flat quadrangular masses of a single layer of cells are found formed in the following manner:—Of two opposite sides the external rows are formed of more or less rounded and angular cells, to which are joined long narrow cells that lie parallel to each other. Those

from each side respectively meet in the centre, where they join. The remaining sides of the quadrangle are formed by a side view of these various cells, where they have been detached from the adjoining ones in the breaking down of the cornea mass. The coincidence between the breadth of the long narrow cells and the fibrillary bundles of the cornea-substance, as seen when prepared by the ordinary methods, is evident, the continuous planes formed by their junction indicating that they form layers between which it is enclosed. According to this view, the ground-substance is everywhere encased in a sheath of cellular elements. Bowman's corneal tubes I believe to include both the straight canals described in the paper above referred to and the spaces between the long cells widened by injection, chiefly the latter. Although I have nothing to add to the description of the mode of preparation which I have already given, I must state that there are conditions of success, as to the nature of which I have not yet come to a definite conclusion. Sometimes the same solution, applied at the same temperature to different corneæ, succeeds in one and fails in another, and sometimes a solution prepared with every precaution has failed to afford me any result. The two essential conditions to success are complete saturation and temperature. I have never succeeded with a temperature above 120°, nor with one below 102°; and so sensitive is the solution to moisture, that preparations sealed in it with asphalte seldom keep longer than one or two days, except in very dry weather. On a damp day I have known a successful preparation left on the object-glass disappear in six hours. The corneal mass may be kept unaltered for at least some weeks in the solution by running sealing-wax round the stopper of the bottle. A perfectly successful preparation shows nothing but the cells. Unsuccessful preparations, especially those prepared with too hot solution, show globular masses unlike any anatomical element; others, especially those prepared at too low a temperature, or with imperfect saturation, show masses of hexagonal crystals like those of cystin. To sum up, I believe that there exists in the cornea:—I., the fibrillary ground-substance, which is pierced by straight canals and honeycombed with cavities; II., flat cells, which everywhere cover the fibrillary bundles of the former and line the entire system of the latter; III., the cornea-corpuscles of Toynbee and Virchow; and IV., the nerve-structures of the tissue. The cornea-corpuscles and the nerves lie free in the canals and cavities, and between them and the epithelium there is a fluid-filled space which permits the passage of lymph-corpuscles. It is therefore proper to regard the canals, cavities, and interfibrillary spaces as forming a continuous and integral part of the lymphatic system, the latter having to the former the same relation that blood-capillaries have to the veins. The junction of the flat cells of the fibrillary substance with the epithelium of the surface justifies the inference that the intercellular spaces in the anterior epithelium of the cornea communicate with the lymph-spaces in the ground-substance, and that the position of nerve-fibrillæ between the epithelium is a continuation of the similar relation that has been demonstrated in the substance of the structure."

A Special Mode of Development in Batrachia.—In a late number of the 'Academy' appeared a notice of some importance on this subject. It says that in a letter printed in the 'Revue Scientifique,' M. Jules Garnier communicates some remarkable observations that have been made by M. Baray on certain Hylodes which exist in large numbers in the island of Guadaloupe. These animals are widely distributed over the island, being found not only near the sea, but in the higher lands of the interior, and after rain their croak makes the air resonant. The physical features of Guadaloupe, a volcanic island, the soil of which is composed of tufa, pozzuolana, and similar material, are so peculiar and so very unfavourable for the maintenance of tadpole life, which is essentially piscine, that M. Baray was led to expect the existence of some peculiarities of development. The ova were easily procured, as they were everywhere present under moist leaves. No tadpoles could be discovered, but many of the frogs were of an extraordinarily minute size. The eggs were spherical, with a diameter of from 3 to 4 millimètres, and were each provided with a small spheroidal expansion resembling a hernia of the gelatinous mass through a pore in the envelope. In the centre of the sphere the embryo was visible, lying on a vitelline mass of a dirty white colour, and having a thin body, a large head and four styliform members with a recurved tail. When the egg was touched the embryo moved rapidly and changed its position. A day later the embryo was perfect, with a tail as long as the body, translucent and like that of a tadpole. The limbs immediately formed, and at the expiration of a few days little frogs of a dark greyish brown colour, and *without a vestige of a tail*, escaped from the egg. M. Baray's observations have established the following facts:—(1) That this *Hylodes Martinicensis* commences life by a rotatory movement of the future embryo. (2) The fully formed embryo performs the rotatory movements more rapidly, but in a horizontal plane. (3) The branchiæ make their appearance, and again vanish sometime afterwards. (4) The larva in the ovum is provided with a tail and limbs. (5) The tail of the larva not only facilitates the movements of the imprisoned animal, but also aids respiration by the numerous and minute vessels which ramify in this highly developed appendage. (6) The animal issues from the egg in the form which it preserves throughout life. As M. Garnier observes, these observations seem to constitute a starting-point for a special investigation of great importance, and have a close relation to the question of the adaptability of species to surrounding conditions. It may be asked in this case whether the frog has been created with special modifications adapting it to live in an island destitute of marshes, or has it in course of time acquired a new mode of development enabling it to survive under the exceptional conditions under which it has been placed.

Discovery of the Position of the Bee's Sting.—Mr. A. S. Packard, jun., makes the following observations in a late number of the 'American Naturalist,' and as they have an important claim to priority of discovery, they deserve a place here. He says that in "Siebold and Kölliker's 'Journal of Scientific Zoology' for July, 1872, containing an account

of the Proceedings of the Zoological Division of the third meeting of the Russian Association of Naturalists, at Kiew, is an abstract of a paper by Ouljanin on the development of the sting of the bee. The author describes but two pairs of imaginal disks, while three were discovered and described by the undersigned in 1866. The author homologizes the elements of the sting with the feet, as had already been done by me in 1871. Soon afterwards Dr. C. Kraepelin published an elaborate article on the structure, mechanism and developmental history of the sting of the bee. In speaking of the origin of the sting,* he only refers to Ganin's observations made in vol. xix. of the same Journal (1869). Dr. Kraepelin seems to have overlooked the writer's papers † on the origin of the sting of the bee and ovipositor of other insects (Æschna and Agrion) published in 1866 and 1868, the observations and drawings having been made in 1863."

The Mouth of the Dragon-fly.—Mr. Packard has also the following note in the same number of the 'American Naturalist' as the above paragraph is taken from :—" An important article on the mouth parts of the dragon-fly, Perlæ and allied forms (*Orthoptera amphibiotica*), is published by Dr. Gerstaecker, in the memorial volume of the Centennial Celebration of the Society of the Friends of Science in Berlin, 1873. The author describes and figures the palpi of the dragon-flies. They possess a one-jointed maxillary palpus, and 2-jointed labial palpus, which are not however in the maxillæ palpiform, but constitute a simple lobe (galea of Burmeister, Erichson and Ratzburg). In Hagen's 'Synopsis of Neuroptera of North America' (1861) it is stated 'mouth not furnished with palpi.' This statement, which is morphologically inexact, was copied in the 'Guide to the Study of Insects.' It will be corrected in the fifth edition of the latter, as it was unfortunately too late to correct the statement in the fourth edition, now passing through the press, except in a few words in the preface."

The Plan of Descent of the Animal Kingdom.—The following is given as a rude outline of the plan sketched out by Professor Haeckel. Regarding the sponges as consisting of two layers of cells, surrounding a body cavity, somewhat as in the Hydra, Haeckel compares the sponge to the embryos of the higher animals, both vertebrate and invertebrate. In his view the germ of all animals, and the adult of such a simple form as Hydra, may be reduced to the simple form of the young of a calcareous sponge which he calls *Gastrula*. " The

* P. 320, vol. xxiii., 1873.

† "Observations on the Development and Position of the Hymenoptera, with notes on the Morphology of Insects," 'Proceedings Boston Society, N. H.,' published May, 1866. "On the Structure of the Ovipositor and Homologous Parts in the Male Insect," 'Proceedings Boston Society, N. H.,' vol. xi., published in 1868. 'Guide to the Study of Insects,' 1869, pp. 14, 536. "Embryological Studies on Diplax, Perithemis, and the Thysanurous genus Isotoma," 'Memoirs Peabody Academy of Science,' 1871, p. 20. Here the spring of the Poduridæ is homologized with a pair of blades of the ovipositor of the bee, &c., and the ovipositor regarded as homologous with the spinnerets of spiders and abdominal feet of myriapods.

Gastrula I consider as the truest and most significant embryonal form of the animal kingdom." It leads in his view to the sponges, to the Acalephæ, to the worms, to the echinoderms, to the mollusks, and to the vertebrates, through Amphioxus. Embryonal forms which may easily be traced from *Gastrula*, occur among the Arthropods (Crustacea as well as Insects). In all these representatives of different stocks of animals, the *Gastrula* always maintains the same structure. From this identity in form of the *Gastrula* with the representatives of the different animal stocks (or sub-kingdoms), from the sponges up to the vertebrates, he imagines an unknown stem-form of animals, typified by *Gastrula*, which he calls *Gastræa*.

Recent Deep-sea Dredgings by the 'Challenger.'—The following extremely interesting letter which was sent from Professor Wyville Thomson to Admiral Richards, has been published by the latter in a late number of the 'Proceedings of the Royal Society.' The Professor says:—"I have the pleasure of informing you that, during our voyage from the Cape of Good Hope to Australia, all the necessary observations in matters bearing upon my department have been made most successfully at nineteen principal stations, suitably distributed over the track, and including Marion Island, the neighbourhood of the Crozets, Kerguelen Island, and the Heard group.

"After leaving the Cape several dredgings were taken a little to the southward, at depths from 100 to 150 fathoms. Animal life was very abundant; and the result was remarkable in this respect, that the general character of the fauna was very similar to that of the North Atlantic, many of the *species* even being identical with those on the coasts of Great Britain and Norway. The first day's dredging was in 1900 fathoms, 125 miles to the south-westward of Cape Agulhas; it was not very successful.

"Marion Island was visited for a few hours, and a considerable collection of plants, including nine flowering species, was made by Mr. Moseley. These, along with collections from Kerguelen Island and from Yong Island, of the Heard group, are sent home with Mr. Moseley's notes, for Dr. Hooker's information.

"A shallow-water dredging near Marion Island gave a large number of species, again representing many of the northern types, but with a mixture of southern forms, such as many of the characteristic southern Bryozoa and the curious genus *Serolis* among Crustaceans. Off Prince Edward's Island the dredge brought up many large and striking specimens of one or two species of Alcyonarian zoophytes, allied to *Mopsea* and *Isis*.

"The trawl was put down in 1375 fathoms on the 29th December, and in 1600 fathoms on the 30th, between Prince Edward's Island and the Crozets. The number of species taken in these two hauls was very large; many of them belonged to especially interesting genera, and many were new to science. I may mention that there occurred, with others, the well-known genera *Euplectella*, *Hyalonema*, *Umbellularia*, and *Flabellum;* two entirely new genera of stalked Crinoids belonging to the Apiocrinidæ; *Pourtalesia;* several Spatangoids new

to science (allied to the extinct genus *Ananchytes*); *Salenia;* several remarkable Crustaceans; and a few fish.

"We were unfortunately unable to land on Possession Island on account of the weather; but we dredged in 210 fathoms and 550 fathoms, about 18 miles to the S.W. of the island, with a satisfactory result. We reached Kerguelen Island on the 7th of January, and remained there until the 1st of February. During that time Dr. v. Willemöes-Suhm was chiefly occupied in working out the land fauna, Mr. Moseley collected the plants, Mr. Buchanan made observations on the geology of those parts of the island which we visited, and Mr. Murray and I carried on the shallow-water dredging in the steam-pinnace. Many observations were made, and large collections were stored in the different departments. We detected at Kerguelen Island some peculiarities in the reproduction of several groups of marine invertebrates, and particularly in the Echinodermata, which I have briefly described in a separate paper.

"Two days before leaving Kerguelen Island, we trawled off the entrance of Christmas Harbour; and the trawl-net came up, on one occasion, nearly filled with large cup-sponges belonging to the genus *Rossella* of Carter, and probably the species dredged by Sir James Clark Ross near the ice-barrier, *Rossella antarctica.*

"On the 2nd of February we dredged in 150 fathoms, 140 miles south of Kerguelen, and on the 7th of February off Yong Island, in both cases with success.

"We reached Corinthian Bay, in Yong Island, on the evening of the 6th, and had made all arrangements for examining it, as far as possible, on the following day; but, to our great disappointment, a sudden change of weather obliged us to put to sea. Fortunately Mr. Moseley and Mr. Buchanan accompanied Captain Nares on shore for an hour or two on the evening of our arrival, and took the opportunity of collecting the plants and minerals within their reach. A cast of the trawl taken in lat. 60° 52′ S., long. 80° 20′ E., at 1260 fathoms, was not very productive, only a few of the ordinary deep-sea forms having been procured.

"Our most southerly station was on the 14th of February, lat. 65° 42′ S., long. 79° 49′ E. The trawl brought up, from a depth of 1675 fathoms, a considerable number of animals, including Sponges, Alcyonarians, Echinids, Bryozoa, and Crustacea, all much of the usual deep-sea character, although some of the species had not been previously observed. On February 26th, in 1975 fathoms, *Umbellulariæ*, *Holothuriæ*, and many examples of several species of the *Ananchytidæ* were procured; and we found very much the same group of forms at 1900 fathoms on the 3rd of March. On the 7th of March, in 1800 fathoms, there were many animal forms, particularly some remarkable starfishes, of a large size, of the genus *Hymenaster;* and on the 13th of March, at a depth of 2600 fathoms, with a bottom temperature of 0°·2 C. *Holothuriæ* were abundant, there were several starfishes and *Actiniæ*, and a very elegant little Brachiopod occurred attached to peculiar concretions of manganese which came up in numbers in the trawl.

"In nine successful dredgings, at depths beyond 1000 fathoms, between the Cape and Australia :—

Sponges were met with on	6 occasions.	*Balanoglossus* were met with on	1 occasion.
Anthozoa Octactinia	7 "	Cirripedia	4 occasions.
" Polyactinia	6 "	Ostracoda	1 "
Crinoidea	4 "	Isopoda	7 "
Asteroidea	8 "	Amphipoda	3 "
Ophiuridea	9 "	Schizopoda	5 "
Echinidea		Decapoda Macrura ..	6 "
Holothuridea	8 "	" Brachyura	2 "
Bryozoa	6 "	Pycnogonida	2 "
Tunicata	5 "	Lamellibranchiata ..	5 "
Sipunculacea	3 "	Brachiopoda	2 "
Nematodes	1 "	Gasteropoda	4 "
Annelida	8 "	Cephalopoda	3 "
(*Myzostomum*)	2 "	Teleostei	6 "

"It is of course impossible to determine the species with the books of reference at our command; but many of them are new to science, and some are of great interest from their relation to groups supposed to be extinct. This is particularly the case with the Echinodermata, which are here, as in the deep water in the north, a very prominent group.

"During the present cruise special attention has been paid to the nature of the bottom, and to any facts which might throw light upon the source of its materials.

"This department has been chiefly in the hands of Mr. Murray; and I have pleasure in referring to the constant industry and care which he has devoted to the preparation, examination, and storing of samples. I extract from Mr. Murray's notes :—

"'In the soundings about the Agulhas bank, in 100 to 150 fathoms, the bottom was of a greenish colour, and contained many crystalline particles (some dark-coloured and some clear) of Foraminifera, species of *Orbulina*, *Globigerina*, and *Pulvinulina*, a pretty species of *Uvigerina*, *Planorbulina*, *Miliolina*, *Bulimina*, and *Nummulina*. There were very few Diatoms.

"'In the deep soundings and dredgings before reaching the Crozets, in 1900, 1570, and 1375 fathoms, the bottom was composed entirely of *Orbulina*, *Globigerina*, and *Pulvinulina*, the same species which we get on the surface, but all of a white colour and dead. Of Foraminifera, which we have not got on the surface, I noticed one *Rotalia* and one *Polystomella*, both dead. Some Coccoliths and Rhabdoliths were also found in the samples from these soundings. On the whole, these bottoms were, I think, the purest carbonate of lime we have ever obtained. When the soundings were placed in a bottle and shaken up with water, the whole looked like a quantity of sago. The *Pulvinulinæ* were smaller than in the dredgings in the Atlantic. We had no soundings between the Crozets and Kerguelen.

"'The specimens of the bottom about Kerguelen were all from depths from 120 to 20 fathoms, and consisted usually of dark mud, with an offensive sulphurous smell. Those obtained farthest from land were made up almost entirely of matted sponge-spicules. In

these soundings one species of *Rotalina* and one other Foraminifer occurred.

"'At 150 fathoms, between Kerguelen and Heard Island, the bottom was composed of basaltic pebbles. The bottom at Heard Island was much the same as at Kerguelen.

"'The sample obtained from a depth of 1260 fathoms, south of Heard Island, was quite different from anything we had previously obtained. It was one mass of Diatoms, of many species; and, mixed with these, a few small *Globigerinæ* and Radiolarians, and a very few crystalline particles.

"'The soundings and dredgings while we were among the ice in 1675, 1800, 1300, and 1975 fathoms, gave another totally distinct deposit of yellowish clay, with pebbles and small stones, and a considerable admixture of Diatoms, Radiolarians, and *Globigerinæ*. The clay and pebbles were evidently a sediment from the melting icebergs, and the Diatoms, Radiolarians, and Foraminifera were from the surface-waters.

"'The bottom from 1950 fathoms, on our way to Australia from the Antarctic, was again exactly similar to that obtained in the 1260-fathoms soundings south of Heard Island. The bottom at 1800 fathoms, a little farther to the north (lat. 50° 1′ S., long. 123° 4′ E.), was again pure "*Globigerina*-ooze," composed of *Orbulinæ*, *Globigerinæ*, and *Pulvinulinæ*.

"'The bottom at 2150 fathoms (lat. 47° 25′ S., long. 130° 32′ E.) was similar to the last, with a reddish tinge; and that at 2600 fathoms (lat. 42° 42′ S., long. 134° 10′ E.) was reddish clay, the same which we got at like depths in the Atlantic, and contained manganese nodules and much decomposed Foraminifera.'

"Mr. Murray has been induced, by the observations which have been made in the Atlantic, to combine the use of the towing net, at various depths from the surface to 150 fathoms, with the examination of the samples from the soundings. And this double work has led him to a conclusion (in which I am now forced entirely to concur, although it is certainly contrary to my former opinion) that the bulk of the material of the bottom in deep water is, in all cases, derived from the surface.

"Mr. Murray has demonstrated the presence of *Globigerinæ*, *Pulvinulinæ*, and *Orbulinæ* throughout all the upper layers of the sea over the whole of the area where the bottom consists of '*Globigerina*-ooze' or of the red clay produced by the decomposition of the shells of Foraminifera; and their appearance when living on the surface is so totally different from that of the shells at the bottom, that it is impossible to doubt that the latter, even although they frequently contain organic matter, are all dead. I mean this to refer only to the genera mentioned above, which practically form the ooze. Many other Foraminifera undoubtedly live in comparatively small numbers, along with animals of higher groups, on the bottom.

"In the extreme south the conditions were so severe as greatly to interfere with all work. We had no arrangement for heating the work-rooms; and at a temperature which averaged for some days

25° F., the instruments became so cold that it was unpleasant to handle them, and the vapour of the breath condensed and froze at once upon glass and brass-work. Dredging at the considerable depths which we found near the Antarctic Circle became a severe and somewhat critical operation, the gear being stiffened and otherwise affected by the cold, and we could not repeat it often.

"The evening of the 23rd of February was remarkably fine and calm, and it was arranged to dredge on the following morning. The weather changed somewhat during the night, and the wind rose. Captain Nares was, however, most anxious to carry out our object, and the dredge was put over at 5 A.M. We were surrounded by icebergs, the wind continued to rise, and a thick snow-storm came on from the south-east. After a time of some anxiety the dredge was got in all right; but, to our great disappointment, it was empty,—probably the drift of the ship and the motion had prevented its reaching the bottom. In the meantime the wind had risen to a whole gale (force = 10 in the squalls), the thermometer fell to 21°·5 F., the snow drove in a dry blinding cloud of exquisite star-like crystals, which burned the skin as if they had been red hot, and we were not sorry to be able to retire from the dredging bridge.

"Careful observations on temperature are already in your hands, reported by Captain Nares. The specific gravity of the water has been taken daily by Mr. Buchanan; and, during the trip, Mr. Buchanan has determined the amount of carbonic acid in 24 different samples—15 from the surface, 7 from the bottom, and 2 from intermediate depths. The smallest amount of carbonic acid was found in surface-water on the 27th January, near Kerguelen; it amounted to 0·0373 gramme per litre. The largest amount, 0·0829 gramme per litre, was found in bottom-water on the 14th February, when close to the Antarctic ice. About the same latitude the amount of carbonic acid in surface-water rose to the unusual amount of 0·0656 gramme per litre; in all other latitudes it ranged between 0·044 and 0·054 gramme per litre. From the greater number of these samples the oxygen and nitrogen were extracted, and sealed up in tubes.

"The considerations connected with the distribution of temperature and specific gravity in these southern waters are so very complicated, that I prefer postponing any general *résumé* of the results until there has been time for full consideration.

"While we were among the ice all possible observations were made on the structure and composition of icebergs. We only regretted greatly that we had no opportunity of watching their birth, or of observing the continuous ice-barrier from which most of them have the appearance of having been detached. The berg- and floe-ice was examined with the microscope, and found to contain the usual Diatoms. Careful drawings of the different forms of icebergs, of the positions which they assume in melting, and of their intimate structure, were made by Mr. Wild, and instantaneous photographs of several were taken from the ship.

"Upwards of 15,000 observations in meteorology have been recorded during the trip to the south. Most of these have already

been tabulated and reduced to curves, and otherwise arranged for reference in considering the questions of climate on which they bear.

"Many specimens in natural history have been stored in about seventy packing-cases and casks, containing, besides dried specimens, upwards of 500 store-bottles and jars of specimens in spirit.

"I need only further add that, so far as I am able to judge, the expedition is fulfilling the object for which it was sent out. The naval and the civilian staff seem actuated by one wish to do the utmost in their power, and certainly a large amount of material is being accumulated.

"The experiences of the last three months have of course been somewhat trying to those of us who were not accustomed to a sea-life; but the health of the whole party has been excellent. There has been so much to do that there has been little time for weariness; and the arrangements continue to work in a pleasant and satisfactory way."

The Enemies of Difflugia.—Professor Leidy remarks * that in the relationship of *Difflugia* and *Amœba* we would suppose that the former had been evolved from the latter, and that its stone house would protect it from enemies to which the *Amœba* would be most exposed. The *Difflugia* has many enemies. "I have repeatedly observed an *Amœba* with a swallowed *Arcella*, but never with a *Difflugia*. Worms destroy many of the latter, and I have frequently observed them within the intestine of *Nais*, *Pristina*, *Chætogaster*, and *Æsolosoma*. I was surprised to find that *Stentor polymorphus* was also fond of *Difflugia*, and I have frequently observed this animalcule containing them. On one occasion I accidentally fixed a *Stentor* by pressing down the cover of an animalcule cage on a *Difflugia*, which it had swallowed. The *Stentor* contracted, and suddenly elongated, and repeated these movements until it had split three-fourths the length of its body through, and had torn itself loose from the fastened *Difflugia*. Nor did the *Stentor* suffer from this laceration of its body, for in the course of several hours each half became separated as a distinct individual."

On the Revivification of Rotifer vulgaris.—In a paper before the Academy at Philadelphia, Professor Leidy observes that during the search for Rhizopods, having noticed among the dirt adhering to the mosses in the crevices of our pavements many individuals of the common wheel-animalcule, *Rotifer vulgaris*, he had made some observations relating to the assertion that they might be revivified on moistening them after they had been dried up. Two glass slides, containing beneath cover glasses some dirt, exhibited each about a dozen active living Rotifers. The glass slides were placed on a window ledge of my study, the thermometer standing at 80°. In the course of half an hour the water on the slides was dried up, and the dirt collected in ridges. The next morning, about twelve hours after drying the slides, they were placed beneath the microscope. Water was applied and the materials on the slides closely examined. On each slide a number of apparently dried Rotifers were observed.

* 'Proc. of Acad. of Sci., Philadelphia,' p. 75, 1874.

These imbibed water and expanded, and some of them in the course of half an hour revived and exhibited their usual movements, but others remained motionless to the last. The same slides were again submitted to drying, and from one of them the cover glass was removed. They were examined the next day, but several hours after moistening them only two Rotifers were noticed moving on each slide. He next prepared a slide on which there were upwards of twenty actively moving Rotifers, and exposed it to the hot sun during the afternoon. On examination of the slide the following morning, after moistening the material, all the Rotifers continued motionless, and remained so to the last moment. From these observations it would appear that the Rotifers and their associates became inactive in comparatively dry positions and may be revived by supplying them with more moisture, but when the animals are actually dried they are incapable of being revivified. Moisture adheres tenaciously to earth, and Rotifers may rest in the earth, like the *Lepidosiren*, until returning waters restore them to activity.—See also 'Silliman's Amer. Journal,' Sept.

New Fresh-water Rhizopods have been recently observed by Professor Leidy, who, in a paper before the Philadelphia Academy, remarked that besides the ordinary species of *Amœba*, which he had observed in the vicinity of Philadelphia, he had discovered what he suspected to be a new generic form. It has all the essential characters of *Amœba*, but in addition is provided with tufts of tail-like appendages or rays, from which he proposed to name the genus *Ouramœba*. The rays project from what may be regarded as the back part of the body, as the animal always moves or progresses in advance of the position of those appendages. "The rays are quite different from pseudopods, or the delicate rays of the Actinophryens. They are not used in securing food, nor is their function obvious. The *Ouramœba* moves like an ordinary *Amœba*, and obtains its food in the same manner. The tail-like rays are not retractile, and they are rigid and coarse compared with those of Actinophryens. They are simple or unbranched, except at their origin, and they are cylindrical, of uniform breadth, and less uniform length. When torn from the body they are observed to originate from a common stock attached to a rounded eminence. Several forms of the *Ouramœba* were observed, but it is uncertain whether they pertain to one or to several species. One of the forms had an oblong ovoid body about $\frac{1}{8}$th of a line long and $\frac{1}{12}$th of a line broad. The tail-like rays formed half-a-dozen tufts, measuring in length about the width of the body. The latter was so gorged with large diatoms, such as *Navicula viridis*, together with desmids and confervæ, that the existence of a nucleus could not be ascertained. The species may be distinguished with the name of *Ouramœba vorax*. A second form, perhaps of a different species, moved actively and extended its broad pseudopods like *Amœba princeps*. When first viewed beneath the microscope it appeared irregularly globular and about $\frac{1}{14}$th of a line in diameter. It elongated to the $\frac{1}{8}$th of a line, and moved with its tail-like appendages in the rear. These appendages formed five tufts about $\frac{1}{25}$th of a line long. The interior of the body exhibited a large contractile vesicle and a discoid

nucleus. This second form may be distinguished with the name of *Ouramœba lapsa.* Another *Ouramœba* had two comparatively short tufts of rays, and a fourth, of smaller size than the others, had a single tuft of three moniliform rays. It is possible that *Ouramœba* is the same as the *Plagiophrys* of Claparède, though the description of this does not apply to that. *Plagiophrys* is said to be an Actinophryen, furnished with a bundle of rays emanating from a single point of the body, but the rays are described as of the same kind and use as those of *Actinophrys*. *Plagiophrys* is further stated to be provided with a distinct tegument like *Corycia* of Dujardin, or *Pamphagus* of Bailey, but the body of *Ouramœba* is as free from any investment as an ordinary *Amœba*, and the rays are fixed tail-like appendages, with no power of elongation or contraction. The species of *Ouramœba* were found among desmids and diatoms, on the surface of the mud at the bottom of a pond, near Darby Creek, on the Philadelphia and West Chester Railroad. Two of the commonest species of *Difflugia* of our neighbourhood I had until recently confounded together as *D. proteiformis*, and, perhaps, the two forms may be included under the latter name in Europe. In one the mouth is deeply trilobed, and the animal is usually green with chlorophyll globules. In the other the mouth is crenulate, usually with six shallow crenulations, and the animal is devoid of chlorophyll. The former is usually the smaller, and may be distinguished with the name of *D. lobostoma;* the latter may be named *D. crenulata.* In an old brick pond, on the grounds of Swarthmore College, Delaware County, among *Difflugia pyriformis*, *D. spiralis*, *D. corona*, *D. acuminata*, and others not yet determined, there occurs an abundance of a large species, apparently undescribed. It is sometimes the fourth of a line in length, and is compressed pyriform, but is quite variable in its relation of length to breadth, and in the shape of the fundus of the shell. This is often trilobate, but from the non-production of one or more or all the lobes, differs in appearance in different individuals. The animal is filled with chlorophyll grains, from which it might be named *D. entochloris.* Another large *Difflugia*, allied to *D. lageniformis*, is not unfrequent about Philadelphia. The shell is beautifully vase-like in shape. It has an oval or subspherical body with a constricted neck, and a recurved lip to the mouth. The body of the shell opposite the mouth is acute and often acuminate. The animal contains no chlorophyll. One shell measured $\frac{1}{6}$th of a line long by $\frac{1}{8}$th of a line broad; another measured $\frac{1}{4}$th of a line long by $\frac{1}{7}$th of a line broad. The species may be named *D. amphora.* A Difflugian, found in a spring on Darby Creek, is interesting, from its transparency, which allows the structure of the animal to be seen in all its details. The investment is membranous and apparently structureless. The soft granular contents occupy about one-half of the investment, and are connected with this by long threads. The pseudopods are protruded in finger-like processes. The form of the animal is compressed ovoid, with the narrow pole truncate and forming the transversely oval mouth. It is probably the species *Difflugia ligata*, described by Mr. Tatem, of England. Its length is about $\frac{1}{33}$rd of a line. The character of the investment is so different

from that of ordinary Difflugians that the species may be regarded as pertaining to another genus, for which the name of *Catharia* would be appropriate."

The Anatomical Changes in Hydrophobia Canina.—A good paper on this subject appears in a late number of the 'Medical Record' (Sept. 30), which says that the long-continued epidemic of last winter has, through the assistance of his colleagues of the Imperial Veterinary School in Vienna, furnished Dr. Benedikt* with numerous preparations from the brain and spinal cord of different animals that had been attacked with rabies. Before describing these, the author discusses the difference presented by the disease as seen in man and in dogs, which has also a special significance with reference to the anatomical appearances. In both the disease begins with a restless melancholia. In the dog this passes into raving madness, while in man this form of mental affection is wanting. In man illusions and hallucinations take but small share in the symptoms, while in dogs they are plainly a prominent feature. In man there is the greatest degree of hyperæsthesia, with highest possible susceptibility for convulsions; in dogs, diffused paralysis and aphonia are among the earliest and characteristic symptoms. In the human being there is the most extreme reflex excitability in the movements of deglutition, so that not only the raising a glass to the mouth, but even the sight of fluids, will induce violent spasmodic action in those organs; whereas in dogs there is a paralysis of deglutition for fluids. In man the severest spasms of the respiratory muscles are present, so severe as sometimes to cause asphyxia. Such spasms are not observed in dogs, which die generally from exhaustion.

Dr. Benedikt has studied the pathological changes by making seven separate vertical sections through the hemispheres in dogs, and has observed such plain and striking pathological changes as could, he observes, only have been previously overlooked by reason of an imperfection of the methods of investigation.

In the first place, there is noted an abnormal distention of the meningeal vessels, and the accumulation around them, and in the meshes of the pia mater, of inflammation corpuscles, together with a nucleolated exudation. This exudation is strongly refractive of light, is colourless, and under high magnifying powers is seen to consist of punctiform nuclear substance (granular disintegration). Striking changes are observed in the grey matter of the convolutions, and in various parts of the nervous centres. One of the coarser changes observed was the presence of numerous holes, or spaces, which, when magnified eighty or ninety diameters, were seen to be filled with a material which also refracted light. This mass, under the high powers of the microscope, consisted of a granular or nuclear substance, in which were single hyaloid and colourless corpuscles, of the size of a distended nucleus of a blood-corpuscle. Inflammatory corpuscles were to be seen in both these masses. In the larger spaces, nerve-cells also were found. Dr. Benedikt further describes what he calls a peculiar condition of the hardened brain, especially in the finer sections. The slightest pressure forced out upon the surface shining masses,

* 'Wiener Mediz. Presse,' June, 1874.

which under the microscope proved to be myelin (colloid? *Rep.*). These masses were often found lying detached on the surface of the section, and presented a greenish lustre. The author states that he has seen the same in the spinal cord of a horse that had suffered from rheumatic tetanus, and that he had regarded it as a softening and chemical alteration of the substance of the spinal cord.

The signs of inflammation are not presented everywhere in the pia mater, but only in certain parts. The distribution of these in the grey matter and in the central white substance throws a new light, according to Dr. Benedikt, upon the nature of the "granular disintegration." (A diagram intended to illustrate this point is given.) From what he has noted, it is concluded that the pathological process in this disease consists in acute exudative inflammation, with hyaloid degeneration, which doubtless arises from the exudative infiltration of the connective tissue. It is characteristic with reference to these inflammatory products that the attack, in man at least, is ushered in with rigors. The hyperæmia and nuclear proliferation is concurrent with that form of diffused inflammation which Lockhart Clarke has designated as "granular disintegration," and so far, the author considers, the anatomical obscurity of this disease is dispelled. The morbid process, in man, is doubtless essentially the same. The usual *post mortem* appearance is congestion and softening, which may have no especial value except as following asphyxia.

Dr. Benedikt states that there are in literature only two trustworthy reports, viz. by Meynert, who found much the same appearances as the author. The spaces, or holes, are regarded by Meynert as being the result of the hardening of the brain-substance. In two other cases Meynert found hypertrophy of the connective tissue in the posterior columns, with molecular and amyloid degeneration in the anterior columns. The nerve-cells of the cortical matter had also undergone partly molecular and partly sclerotic change.

NOTES AND MEMORANDA.

The Method of Measuring Angular Aperture.—The 'American Naturalist' (August) states that Mr. Wenham, in order to gain accuracy in measuring the angular aperture of dry objectives, would like to cut off all stray light that might enter the lens without being capable of forming an image, by placing over the objective a conical nozzle having a small aperture in its apex. This aperture would correspond to the focus of the lens, and the nozzle would just include the cone of rays capable of forming an image, and would exclude all false rays of any considerable angle. This method would be inconvenient, however, and as the angle is measured by a horizontal movement, a vertical slit will be a satisfactory substitute. For high powers the slit must have thin edges; and it must be capable of adjustment to the width of focus of the lens. His arrangement is easily made and

used. A plate three inches long and one inch wide has a central aperture nearly one-half inch wide, the edges of this opening being bevelled away below so as to admit a large angle of light. Upon this plate lies a glass slip about 2 in. × ½ in., pressed against at one end by a spring, and at the other end by a screw, so that it can be easily slid backwards and forwards under the two staples (one inch apart) which hold it upon the surface of the plate. The slip is formed by the edges of two slips of platinum foil (·001 thick) one of which is cemented with Canada balsam upon the glass slip, while the other is fastened under one of the staples so as to lie on the glass slip but not move with it. These platinum slips never overlap; but their edges may be brought in contact, or may be separated as widely as desired by means of the set-screw pressing against one end of the glass slip which carries one of them. In measuring angles the usual method of rotating the instrument horizontally is employed; only this apparatus lies upon the stage with its slit in focus of the objective, and adjusted in width so as barely to include the whole breadth of the focus. If the stage of the microscope is too thick to admit full angle of light, the apparatus may be arranged below the stage, and the objective focussed down to it.

A New Microscopical Society has, we are glad to perceive, been established at Memphis, Tenn., U.S.A. Its bye-laws bear date August 28, 1874, and are terse and to the point. It numbers about twenty-six members, Dr. Cutler being its President, and Mr. S. F. Dod its Secretary and Treasurer. We wish it every success, and hope to have some contributions to our Society from its members.

The Leeds Public Library.—We have much pleasure in acceding to the Librarian's wishes—making known to those of our readers who may reside in the neighbourhood of Leeds the fact that the Leeds Public Library contains an admirable and considerably large selection of Natural History and Microscopic works. It is a public, and therefore a free, library, and its catalogues are published in separate sections; that which we have seen being devoted to natural history, or biology in its widest type. We notice in it some few errata, which we hope to see removed as soon as possible. Who, for instance, is E. Lambert, who is stated to be one of the Editors of the 'Quarterly Journal of Microscopical Science'?

CORRESPONDENCE.

Beneche's No. 7 Objective.

To the Editor of the 'Monthly Microscopical Journal.'

Norwich, *October* 8, 1874.

Sir,—Having recently obtained one of Beneche's No. 7 objectives and submitted it to one or two sharp trials, I am able to confirm Mr. Hickie's report of the performance of these glasses. Mr. Hickie says the one he tried was equal to a "weak English ⅛." Mine I should call

a strong English quarter; on comparing its magnifying power with a $\frac{1}{4}$ inch made in 1843 by A. Ross (of 75° angular aperture only, but a most excellent glass), and using a B. ocular, I found the $\frac{1}{100}$ division on the micrometer (when $\times$ 400 diameters) was exactly $\frac{1}{8}$ of an inch longer when magnified by the No. 7 than when the quarter was used. The angular aperture of Beneche's lens is certainly not less than 95°. And now for what I have been able to do with it.

Pleurosigma angulatum mounted by Möller (which by the way is much more robust than the species we find in England), striæ resolved by direct light; with oblique light and D. ocular, the peculiar arrangement of the terminal striæ were well shown.

P. intermedium, striæ easily seen by oblique illumination.

Nitzschia sigmoidea, transverse striæ very fairly distinct.

Cymbella Ehrenbergii, the costæ were easily resolved into compressed beads, as were also the transverse markings on *Synedra robusta*. *Pinnularia peregrina*, the fine transverse lines on the costæ were plainly visible (the last three specimens were in balsam). This glass also performs well with the binocular, both tubes being well illuminated.

The admirable performance of these glasses is remarkable, as they have no adjustment, and the price in Berlin is 10 thalers!!!

Yours truly,

FRED. KITTON.

POWELL AND LEALAND'S $\frac{1}{8}$TH AND $\frac{1}{4}$TH WITH STRAIGHT CANDLE-LIGHT.

To the Editor of the 'Monthly Microscopical Journal.'

YORK, *October* 7, 1874.

SIR,—Your correspondent, Mr. Hickie, desires to know if any of your readers can do with *their* $\frac{1}{4}$th objectives what he can do with *his*, that is, whether they can resolve the markings on "*Pleurosigma angulatum*" in a perfectly satisfactory manner under certain severe restrictions which he mentions. If, therefore, you will allow me, I will briefly state how a $\frac{1}{4}$th in my possession has behaved under these limitations.

But since he speaks of the performance of his Gundlach's $\frac{1}{8}$th, it may be as well to inform you first how a similar power, likewise in my possession, fared under the ordeal to which both were subjected. My glasses were obtained from Messrs. Powell and Lealand, the eminent London makers.

The slide of "*P. angulatum*" selected was one covered dry for $\frac{1}{25}$th inch. This I placed upon the stage, and then brought the flame of a composite candle to an exact level with it, and precisely in a line with the tail of the compound body, mirror and diaphragm being both discarded. The first frustule that appeared now stood out with such clearness, and was so beautifully defined, that the whole proceeding, when viewed as a testing operation, looked to me to be little better than child's play.

Afterwards I brought the ¼th to bear upon the same object, illumination and everything else remaining precisely as before, except that the B eye-piece now became almost a necessity. On proper focussing the resolution of the striæ quite surpassed my expectations: the definition was crisp and little short of brilliant.

As Mr. Hickie does not state what his Berlin ¼th has done with "*S. gemma*," comparison is of course impossible; but if he means that it has detected the longitudinal lines of that diatom, it would be a real favour to microscopists to tell them of the feat. He is doubtless too experienced an observer to be misled by the deceits of diffraction.

I am, Sir, your faithful servant,

R. Corbet Singleton.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *October* 7, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting in June was read by the Secretary, and the thanks of the Fellows were voted to the donors.

Mr. H. J. Slack called special attention to some of these donations, consisting of some curious old microscopes and optical apparatus presented to the Society by Dr. John Gray; amongst which were an ancient microscope of unknown date, and an elaborately made instrument by Tully, for Robert Brown, also some spectacles, with a series of lenses of different powers, which were used by the great botanist in his researches.

Mr. Hembry, introduced by Dr. Braithwaite, was presented to the President and formally admitted as a Fellow of the Society.

A paper by Mr. Alfred Sanders, entitled "Supplementary Remarks on the Appendicularia," was read by the Secretary. The paper was illustrated by drawings, and appears at p. 209 in the present number of the Journal.

Mr. Slack said that a few months ago Mr. H. R. Webb of Lyttleton, N.Z., one of the Fellows of their Society, sent over some samples of earth containing diatoms, &c. These were placed in the hands of Mr. Kitton, and that gentleman had discovered amongst them a new species of *Surirella,* which he had described as *S. contorta.* A paper by Mr. Kitton, descriptive of the forms found in these deposits, and also amongst some dredgings sent by Capt. Parry from Colon, Panama, was then read by the Secretary, and the illustrative drawings were handed

round for the inspection of the Fellows. The paper is printed at p. 218.

Votes of thanks to Mr. Sanders and to Mr. Kitton for their communications were unanimously passed.

Mr. Slack remarked that the danger of naming objects upon insufficient information received fresh illustration from the paper, in which mention was made of a species of Triceratium having seven points.

Mr. Slack said he had brought to the meeting some films of silica prepared from a solution containing a mixture of one part of water and four parts of glycerine. Some of them were exceedingly delicate, and it was not possible to get good definition of them with high power objectives of large angles. When seen with a glass of large angular aperture there were so many false images that the true effect of what ought to be seen was entirely lost. He had, however, been able to obtain some good definitions with an object-glass, $\frac{1}{8}$th, 60° aperture, used in conjunction with Mr. Wenham's dark-ground illuminator. It was of interest to know that in searching for minute particles of a highly refractive nature they could not always be detected with a high-angled lens. Most microscopists would still persist in using these objectives for all purposes, in spite of what had been pointed out by Dr. Carpenter, Mr. Brooke, and others. By using a small-angled glass they would often see much more, and when Mr. Wenham's illuminator was also used, effects would be obtained which seemed to be unattainable in any other way.

Mr. Charles Stewart called the attention of the meeting to a very curious living organism which was exhibited in the room by Mr. Badcock. He then drew it upon the black-board, and gave a general description of its appearance, expressing a hope that all who were present would avail themselves of the opportunity of seeing it. He was quite unable to say what it was, but from its general appearance he thought it was something like an entozoon.

Mr. Badcock, in reply to a question from the President, said that the creature was developed in his aquarium in the month of June last. He thought at first that it must be the larval condition of some other form, but it did not seem to have undergone any change in the course of four months. The only suspicious thing in the aquarium was a fresh-water mussel, he did not know whether that had anything to do with it. He had brought some sketches of it, which were placed upon the table for inspection.

The President invited information upon the subject from the Fellows present, who he hoped would examine it and say if they thought it to be a larval condition, or a perfect animal, or what?

Mr. Slack said that in its extraordinary power of changing its shape it resembled *Bucephalus polymorphus*,* an entozoon found in fresh water, but then it was unlike it in other respects.

The President directed the attention of the Fellows to a remarkable collection of photographs of animal tissues and morbid conditions of the same, which had been presented to the Society by the Army Medical Department, Washington.

* Prof. Reay Greene has since seen the animal, and considers it a Bucephalus.

The proceedings then terminated, and the meeting was adjourned to November.

Donations to the Library, &c., since June 3, 1874:—

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*Society.*
Transactions of the Linnean Society. Two parts	*Ditto.*
Journal of the Linnean Society, No. 76	*Ditto.*
Popular Science Review, Nos. 52 and 53	*Editor.*
Journal of the Geological Society, No. 119	*Society.*
Smithsonian Report for 1872	*Institution.*
Bulletin de la Société Botanique de France. Three parts	*Society.*
The Toner Lectures. By Dr. J. J. Woodward. Illustrated with seventy-four Photographs	*The Surgeon-General, U.S.A.*
A simple microscope and set of powers by Tully	
A single microscope by Dolland	
An ancient microscope and some spectacles that were used by the late Mr. Robert Brown	*Dr. John E. Gray, F.R.S., &c.*
One slide of silica film	*H. J. Slack, Esq.*

WALTER W. REEVES,
Assist.-Secretary.

QUEKETT MICROSCOPICAL CLUB.

Annual Meeting, July 24.—Dr. Braithwaite, F.L.S., President, in the chair.

The Ninth Annual Report of the Committee was read, giving details of the work accomplished by the club during the past year, and testifying to the continuance of its prosperity, and successful progress in the course marked out by its founders. The President read the Annual Address, in the course of which he made some interesting remarks upon the function of the various organs of plants, tracing the chemical and physiological changes which took place during the germination of the seed and the development of the plant, thus adding a supplement to the valuable and interesting series of papers "On the Histology of Plants," written by him for the club.

Officers for the ensuing year were elected, the President being Dr. John Matthews, F.R.M.S., who was duly inducted by the retiring President.

Dr. W. Sharpey, F.R.S., &c., was unanimously elected an honorary member of the club, and four ordinary members were also elected.

Ordinary Meeting, August 28.—Dr. Matthews, F.R.M.S., President, in the chair.

Three members were elected.

Extracts were read from a letter written by Mr. S. Green, of Colombo, to Mr. T. Curties, giving further details of his methods of mounting insects without pressure, so as to preserve their natural appearance, and a further communication on the subject was promised.

There being no paper, Mr. Ingpen made some remarks upon two gatherings of *Volvox*, both obtained on the 1st of January last, one of

which furnished good specimens as late as the beginning of April. The other gathering was slightly frozen in February, shortly after which Statospores were found in most of the conditions described by Dr. J. Braxton Hicks, in the 'Quart. Journ. Mic. Science,' vol. i., N.S., p. 281.

The President announced the excursions and meetings for the ensuing month, and the meeting closed with the usual conversazione.

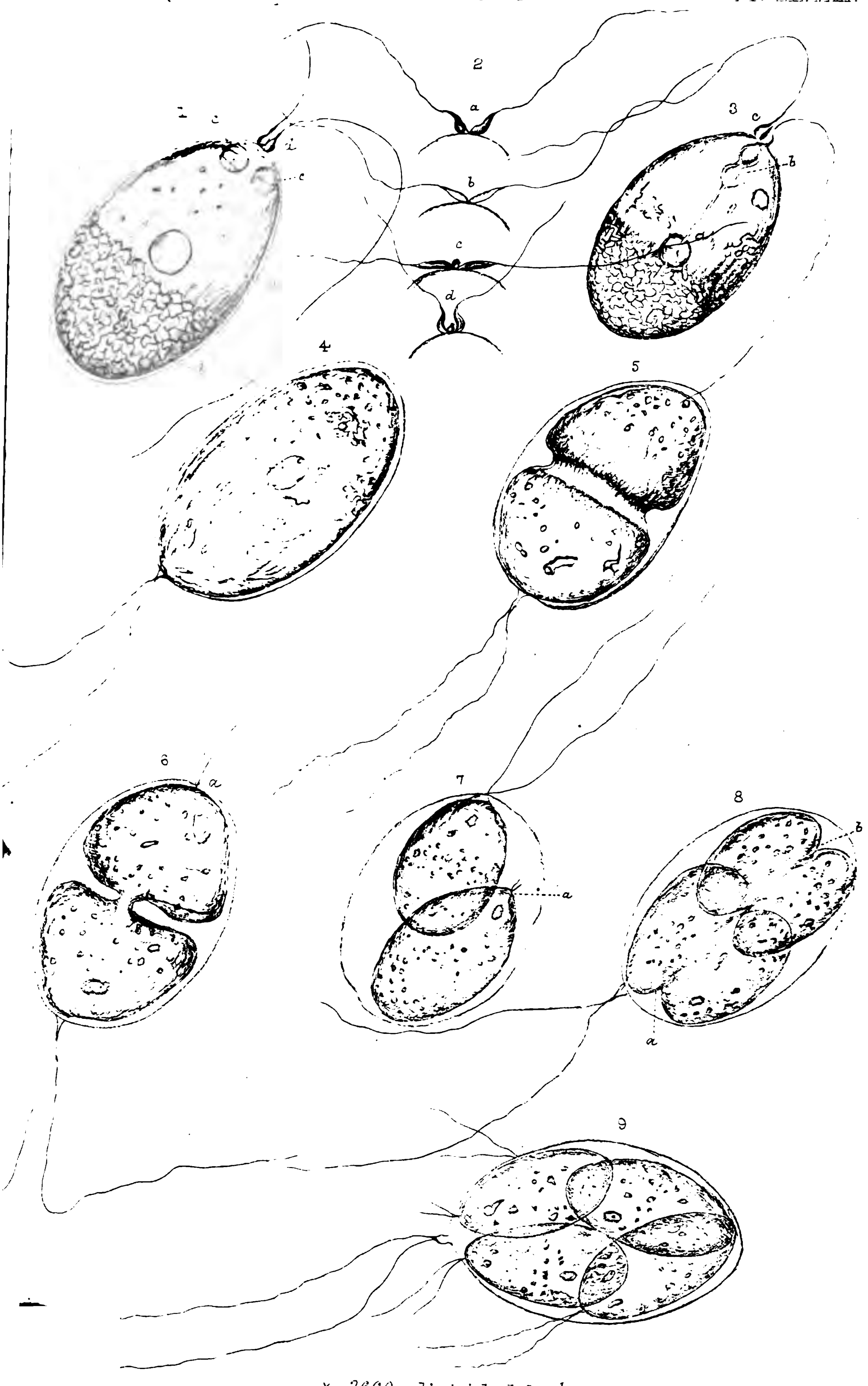
Sept. 25.—Ordinary Meeting. Dr. Matthews, F.R.M.S., President, in the chair.

The President brought to the meeting for distribution a quantity of infusorial earth from several localities in Barbados, which promised to be rich in Polycystina.

Mr. T. Charters White read a paper "On the Salivary Glands of the Cockroach," illustrating the subject by coloured diagrams and beautifully prepared specimens. He first spoke of the structure and functions of glands in general, and then showed the method of dissecting the salivary glands from the cockroach, which he very minutely described, and added the reasons which confirmed him in his opinion as to their possessing the nature and functions of true salivary glands. Mr. T. Curties read a letter which he had received from Mr. J. G. Tatem, accompanying a slide of the same object for presentation to the club. In this letter Mr. Tatem adopted the opinion advanced by Mr. Hollis in a recent number of 'Nature' that the sacs are not reservoirs for saliva, but air-sacs only, and probably capable of inflation as an aid to flight. He could not exactly comprehend in what way the sacs could be filled with air from without, but thought that they might possibly be inflated from time to time with "*secreted air*," as in the case of the bladders, having no ducts, of some fishes.

In the discussion which followed, Mr. Lowne supported Mr. White's view, and suggested staining the preparations with chloride of gold, for the better demonstration of the network of nerves. He spoke at length on the different kinds of salivary glands in vertebrate and invertebrate animals, and stated that there was no evidence of the sacs being tracheal tubes, or of their ever being filled with air. The President read Dr. Hollis's letter in 'Nature,' and said that he did not regard the presence of trachea as conclusive of the argument, and thought that on the whole the reasoning was inconclusive.

Mr. Loy felt quite sure that the tubes were not tracheal, and thought that the sacs were reservoirs in which the saliva was stored until pumped by the tube into the stomach. He had never found any evidence of air in any insect's salivary glands. In answer to a question from the President, Mr. Loy made some remarks upon the irritating nature of the saliva of some insects, and afterwards suggested different ways of killing the cockroaches, which Mr. White proposed to dissect at the next conversational meeting for the instruction of the members. After the meeting, several beautiful preparations of the disputed organ were shown, as well as other objects of interest, by various members.



x 2600 diminished by ½.

W. West & Co. lith.

THE

MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1874.

I.—*Continued Researches into the Life History of the Monads.*

By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D., F.R.M.S.

(*Read before the* Royal Microscopical Society, *Nov.* 4, 1874.)

Plates LXXXIII., LXXXIV., and LXXXV.

The prosecution of our inquiries as to the developmental history of the minute monad-forms found in macerations of fish was continued during the past summer. Our methods were precisely the same as those previously described; so also was our mode of work, everything being mutually accepted as a correct interpretation.

Prolonged work with infusions has led us to make observations

DESCRIPTION OF PLATES LXXXIII., LXXXIV., AND LXXXV.

Fig. 1.—Typical specimen of the monad described.
„ 2.—Shows the different positions taken by the enlarged bases of the flagella when the latter are in motion.
„ 3.—The peculiar structure *a*, *b*, intimately connected with the bases of the flagella *c*, indicates a probable organ of locomotion; of which *b* may be muscular.
„ 4.—The earliest condition of change of state, showing hyaline investment more clearly.
„ 5.—First constriction within the hyaline.
„ 6.—Constriction more developed.
„ 7.—The first division into two complete; but the forms are still within the hyaline.
„ 8.—Transverse constriction ensues in each of the preceding.
„ 9.—Division into four has taken place, and the flagella of some of the enclosed ones are protruding.
„ 10.—Further multiplication has ensued, and the first of the new forms is escaping from the hyaline investment.
„ 11.—The hyaline envelope after all have escaped.
„ 12.—The same when one or more has aborted.
„ 13.—A specimen of the granular form from which parthenogenetic elements are emitted.
„ 14 and 15.—The emission of the above.
„ 16, 17, and 18.—The condition of the monad after emission.
„ 19 and 20.—The germinal points seen in the granules of sarcode emitted, as discovered in the process of development. × 10,000 diam.
„ 21.—Sexual contact.
„ 22.—The earliest result of the blending.
„ 23.—The still condition which follows.
„ 24 and 25.—The cyst bursting and emitting a cloudy mass containing germs.
„ 26 and 27.—Represent the "clubbed" condition.

concerning them which, although without explanation or apparent bearing at present, seem to us of sufficient importance for note. Our first maceration was a cod's head; it was freely exposed to the air, but excluded from the light. For two months nothing at all remarkable presented itself. Abundance of *Bacteria termo*, *B. lineola*, and Amœbæ were found. But at the expiration of the twelfth week the form to be described in this paper gradually appeared—survived for three months and two weeks, to the almost complete exclusion eventually of other forms—and then was supplanted by other monads, some of which have been described by us in former papers.

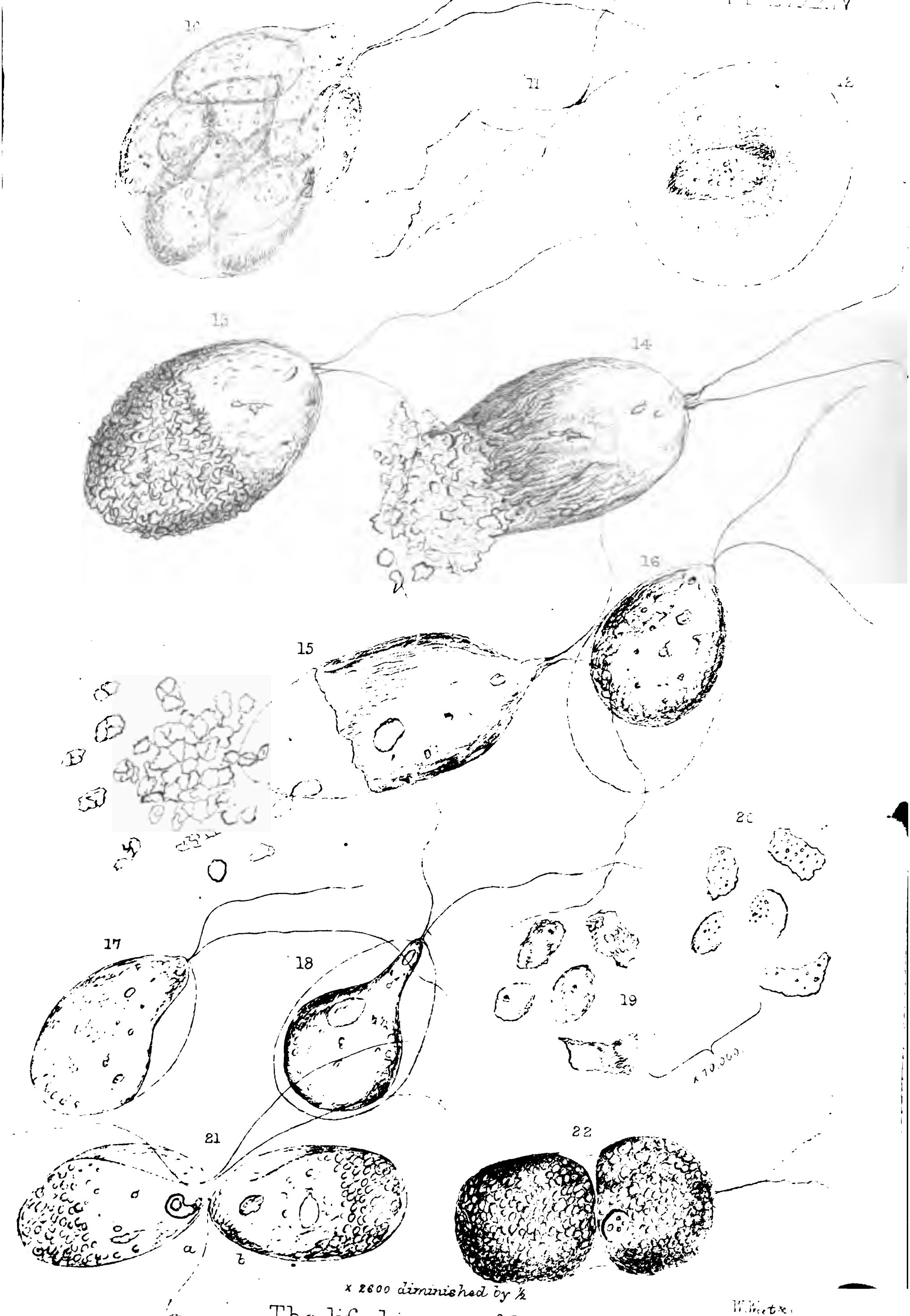
This maceration was made from ordinary water supplied by the company on the Cheshire side of the Mersey. The same year, in the same place, another cod's head, and the head of a salmon were macerated in separate vessels. It was later in the year, and the production of vital forms was slower; yet in the course of four months the same phenomena as those described above took place; the only difference being that the form that we are about to describe did not persist so long.

In the autumn of the same year another cod's head maceration was made in Liverpool from the ordinary water supplied to the town. This up to the spring of the following year showed no trace of the form in question, nor indeed of any monad, but swarmed persistently with gigantic specimens of the *Spirillum volutans*. After this several other macerations were made, in the same place, and the form we desired appeared, but no spirilla could be discovered. While a maceration of salmon's head made in April, 1873, under the same circumstances at the same place (viz. on the Lancashire side of the Mersey), was found in April, 1874, to swarm with the peculiar monad form in question; but another infusion of herring made at Rock Ferry (on the Cheshire side) late in the summer of 1873 has not yet shown the monad we hoped for. What determines their appearance or non-appearance we have no data even to surmise; but it is a subject which is securing our attention.

Another incident in our last summer's work may be mentioned. We always work from a small quantity of the large vessel of decaying matter which we can keep at hand. During the early summer the intense and continued heat evaporated all the fluid from the salmon's head infusion without our knowledge. The form at which we were working had been in it in great profusion. It was growing less abundant in our small working tank, and we feared that we must wait another year to finish our inquiries. But we led a forlorn hope, and took the hard, porous, dried, papier-maché-like mass which formed the dry residuum of the infusion, and determined to put it into an exhausted maceration of the same

The Monthly Microscopical Journal. Dec. 1. 1874.

Pl. LXXXIV.



The life-history of Monads.

W. West & Co. lith.

× 2600 diminished by ½.

The life history of Monads.

kind at Rock Ferry, which at the time showed only very feeble signs of *any* life, and certainly no monads. We watched the result; and, to our great surprise, in three days the required monad appeared in remarkable vigour and daily increasing abundance, enabling us to complete our researches into its cycle of development. Whilst at the same time another, and remarkable form, whose history we have since completed, and which had only very feebly shown itself before the drying up of our infusion, now showed great vigour, and eventually survived and predominated, evidently very much at the expense of the former.

This may be accounted for in two ways at least. First, the germs—which we have proved to exist in the development of this form—gave origin to new monads when the caked mass was broken up by solution and set them free in normal conditions; and second, we are strongly inclined to believe that hundreds of millions of the adult forms were only *desiccated* by the drying up, and were resuscitated when the fluid was restored; an opinion which subsequent experiment has done much to confirm.

With reference to the immediately asserted, and eventually absolutely secured ascendency of the *new* form above referred to, after the remoistening of the dried maceration, it is evident that some new conditions had arisen in consequence of the drying up of the pabulum, and its subsequent remoistening, which in the struggle for existence made it the fittest to survive.

The form we now describe has occupied our attention for at least three years, but some points of difficulty each year presented themselves, and we have delayed any reference to it until we had learned as much concerning it as we deemed possible. It is in fact the form incidentally referred to in our first paper,* and drawn, in various positions, at *a, a,* &c., Fig. 1, Pl. XXIV., vol. x., of the 'Monthly Microscopical Journal.'

It is a form possessed of more distinctive and distinguishable structure than any other so low in the scale of life with which we are acquainted. Its most marked peculiarities may be summarized with the aid of Fig. 1. The sarcode is invested with a distinct hyaline envelope, perfectly structureless to our best appliances, and sharply distinguishable from the protoplasm of the body; two flagella, inserted into what appears like a special organ of locomotion; a large central disk or nucleus-like body, *a*; numerous protoplasmic granules, *b*, the function of which we shall shortly explain; a pair of "snapping" eye-spots, *c*;† and occasionally some remarkable club-like appendages to the anterior part of the body, the nature of which we have failed to ascertain; and to which we shall again refer.

The body is oval, the pair of flagella with which it is furnished

* 'M. M. J.,' vol. x., p. 53.
† Ibid., vol. xi., p. 8.

being placed anteriorly. The average length of the long diameter of the body is the 1100th of an inch. The flagella are about twice the length of the body, very fine, and intensely rapid and graceful in their movements. They are inserted into a couple of pear-shaped bodies, with their thickest ends in contact with the investing membrane. They are shown as they generally present themselves in Fig. 1, *d*, but they also assume the conditions drawn in Fig. 2, being intimately connected with the motion of the flagella; this may be distinctly seen when the motion of the monad becomes slower. *a*, *b*, *c*, and *d* show some of the positions assumed, and their relations to the movements of the flagella.

The motion of this monad when in complete activity is extremely graceful, almost swallow-like; but there is no question left upon our minds that it is wholly accomplished by means of the flagella. They are usually thrown out in the manner of a swimmer's arms, and made to meet at the posterior end of the monad; but they can also be used in all directions, either singly or together, giving either a rolling forward motion, or a gyrating horizontal motion, or even a longitudinal revolution. They can also move backwards, by uniting the flagella and making a sculling motion.

To attempt to give anatomical explanation of their movements as produced by what appears to be a mere mass of structureless sarcode would be waste of space. But we are constrained to indicate what was seen twice by both of us, and three times by one, as indicating something that suggests structure. We were observing once with the $\frac{1}{50}$th, and once with the $\frac{1}{25}$th, when we perceived by careful focussing what is drawn in Fig. 3, where the rod *a* seemed to run longitudinally through the monad as if for support; the bulbous part *b* was closely connected with the knobs *c*, which give actual support to the flagella.

The posterior part of the sarcode is always filled with granular masses of protoplasm to nearly the extent of half the body, as seen in Fig. 1. These, as we shall subsequently see, play an important part in the life history of the creature. Immediately above this granular mass is situated a nucleus-like body; it is without structure, and large in proportion to the size of the monad, always occupying the same position, *a*, Fig. 1. Beside these peculiar features this creature possessed almost constantly the snapping eye-spots which we have shown to belong to other monads, and have fully described in earlier communications,* but the function of which we have failed to discover.

We may now consider the phenomena attending the developmental history of this form, which is divisible into three chief features.

(1.) By continuous observation on the normal form, with a

* 'M. M. J.,' vol. xi., p. 8.

power of from 1200 to 10,000 diameters, the fine hyaline investment in the initial stages of development is perceived more clearly, enveloping the monad, but no change of shape or motion ensues, Fig. 4. In about forty minutes to an hour a line suddenly appears across the short diameter of the oval, which soon develops into a very marked constriction, as seen in Fig. 5. This constriction continues rapidly to increase within the hyaline membrane, which throughout the process preserves its normal form, until it reaches the condition drawn in Fig. 6. During the whole of this time the motion of the monad is unaffected; and in about two hours from the first* a total division takes place. Just before division, however, in some way not made out, two short cilia appear in the place of the future flagella, as seen in *a*, Fig. 6; but directly actual division takes place the separated monad turns over, and occupies the position seen in *a*, Fig. 7. After swimming freely in this condition for not less than ten minutes an indentation may be observed in the long axis of the divided bodies within the hyaline, and in from seven to twenty minutes a constriction longwise ensues,† as seen in Fig. 8, where *a* and *b* show the lines of constriction. After this the divided bodies remain within the hyaline envelope, sometimes dividing into eight and even into sixteen, although rarely, and swim about with an elegance and ease certainly not surpassed by the pregnant *Volvox globator*. Generally this compound mass is dependent for motion on the original flagella of the original monad, which persist throughout; but at times, determined by conditions we have not discovered, the flagella of each new form protrude beyond the hyaline envelope, as seen in Fig. 9, but these always move in concert, and apparently obey a common impulse. After swimming in this way for a length of time, varying from ten to one hundred minutes, or more, one of the forms within the hyaline investment protrudes itself, as seen in Fig. 10, and shortly escapes a perfect monad like its parent. This is repeated in each case until in the majority of instances all escape, leaving the fragment of a pellicle or sac behind with the old flagella attached, as drawn in Fig. 11. But in many cases there appears to be incapacity to throw off the last one or two, and it remains apparently dead, as seen in Fig. 12. This is the usual method of increase, and goes on with great rapidity; the multiple forms in a fresh field, always bearing a large proportion to the other forms. This process does not terminate with the first generation so produced, but may be continued for many generations in succession with no congress of any kind and no visible modifications.

(2.) But the attentive and patient observer will soon find

* Sometimes it is much quicker, and at other times much slower than this.

† This is not invariable: sometimes the constrictions are all along the short diameter.

himself arrested by another kind of phenomenon. Some of the normal forms become *extremely* granular at their posterior or non-flagellate end, so that the granules give the protruding effect of an acorn cup. These swim with great freedom, and are generally larger than the other forms. One of them is represented at Fig. 13. Suddenly, and without warning, these swiftly moving bodies shoot out almost the whole mass of granules, and deposit them, as seen in Figs. 14 and 15, leaving the monad almost entirely destitute of granules, and with the hyaline membrane still retaining its shape, but the sarcode within much altered in form and position. This is shown in Fig. 15; but also other modifications attending the emission are drawn at Figs. 16, 17, and 18. At certain stages of development thousands of these granular forms are visible in every "dip" of the fluid. At first the extruded granules seem to have no significance; and they were for a long time a source of great perplexity to us. But we confined our attention at length wholly to these for some time, and by the use of our best appliances were enabled to discover their nature. When deposited, the granules are amorphous, more or less agglomerated, and perfectly transparent. Watching them attentively with the highest available powers of the $\frac{1}{50}$ we at length saw spots, or minute points or dots, appear in the granules, as seen in Fig. 19. These under constant observation increased, and in one of them, as many as seventeen were counted. In this condition they are drawn at Fig. 20. They remained like this for from two to three hours, only slightly increasing in size. At the expiration of this time a vibratory motion of the internal points was perceived, which very rapidly increased, and in the course of forty minutes intense internal activity was visible, the minute dots within the sarcode moving upon each other in all directions. This lasted from ten to fifty minutes, when they all escaped and at once swam freely as minute bacterial-like bodies, but no trace of any organs of locomotion could be discovered. After they began to move they rapidly increased in size, so that in from four and a half to five hours they were of normal size, and endowed with all the powers of the original monad. This was seen again and again, in all its stages, and the new forms were followed up to the condition of multiple fission, as before described.

Other phenomena presented themselves, but nothing that we could explain or correlate; and we were for two years inclined to think that this must be the entire process of development. But commencing again with fresh working power, we were this last summer enabled to find what gives completeness to this history.

(3.) We had occasionally seen during the whole period of our researches on this form a coming together of two of the monads; but from its infrequency and occasional abortiveness, as well as

from the prominence of other phenomena, we did not with any continued intensity follow it up. This past summer we made it a specific object, and by dint of close application found that, in comparison with other phenomena, very occasionally two of the monads, at times in no way distinguishable from each other, met and touched each other at their anterior ends, swimming freely together, as seen in Fig. 21. The normal flagella rapidly disappeared, and the bodies melted into each other; another stalked double flagellum appearing at one end in a manner never in any way understood by us. The nuclear bodies *a*, *b*, Fig. 21, blended also into one; the whole thing in this stage being shown at Fig. 22, where also an intensely granular state peculiar to this condition is shown. This body preserved its freedom of motion for a long time —occasionally for ten or twelve hours—but during this time it lost slowly the line of juncture, and became oblong and then rounded; after which its motion was more sluggish, and it eventually became quite still. Fig. 23 represents it in this condition. It remained in this state sometimes as long as twenty-four hours; but generally, from four to six hours was the time occupied before any changed ensued. The uncertainty, however, made constant watching absolutely necessary. The whole sac showed as the first symptom of change a slight vibration or wave-like motion, and then, with no further premonition, its edges broke up and a cloudy mass poured out, in which with competent powers it was comparatively easy to detect myriads of minute points. Fig. 24 depicts this; and by following up rigorously these emitted points, we found that after a short period of inactivity they became motile, and rapidly grew, acquiring flagella, and becoming perfect monads of the parent type. Not only so, but these very forms were persistently followed, and were seen to increase by multiple fission, and to deposit granules as before described. At times the globular condition was not taken, but the emission took place in the condition shown at Fig. 25. It will be seen that this, and another form which we hope to describe in a subsequent number, gave us more trouble and perplexity than any we had worked at. But after working the whole life cycle out it now appears to us that this monad primarily multiplies sexually by the congress of the genetic elements. This, however, is comparatively a very rare occurrence, and serves for many (probably) hundreds of generations. But a kind of parthenogenesis, or internal budding, follows—resulting in the emission of the sarcodic granules which contain minute monad-germs—this being by far the more frequent and rapid mode of increase; while at the same time multiple fission is taking place in all directions.

Thus we have the minute germ sexually produced; the bud produced in large quantities within the unfertilized form, both

of which *grow* to the parent size; and the perfect series of monads produced directly by multiple fission.

From the peculiar manner in which the parthenogenetic products are deposited—in a clear investing sarcode—the capacity for desiccation so remarkably shown by this monad may be understood on the principle pointed out by Mr. Henry Davis.*

There is one condition of this monad which, in spite of most constant and assiduous research, has defied all our attempts to discover its meaning. We have called it the "clubbed" stage, for in this special condition the monad was vested with one or two peculiar knobbed stalks either supplanting or associated with the flagella. The ordinary clubbed condition is shown at B, Fig. 26, where *b*, *c* appear to have taken the place of the ordinary flagella. Almost as frequently the condition seen at A is assumed where there is one flagellum and one knob; but instances have often occurred in which both flagella and two knobs exist together as at *c*.

We have endeavoured for three years to find the meaning of this, but have entirely failed. We persisted in our efforts, because, so far as we could discover, this anatomical phase seemed to coincide with certain stages of development. But wider and closer observation has enabled us to abandon this idea. Our first impression was that this phenomenon had a sexual significance, and this arose from the fact that we had frequently observed copulating forms, as at *c*, Fig. 21, in which one of the monads was clubbed. But from the large number of cases subsequently watched with all conceivable care, in which no such a phenomenon presented itself, we are obliged to abandon this also. That it is without significance in the creature's development we are unwilling to think; the more because of its occurrence each year, and with greater or less persistence throughout, as well as on account of the occasionally observed method of its production. In Fig. 27 the mode of origin is shown. At first two disks were seen within the sarcode, as in *a*, *b*, A; these would slowly push out, as in *c*, *d*, B, and the stalks would appear, and eventually they would be wholly and permanently thrown out, as in *e*, *f*, C.

But in spite of this we have failed to correlate it with any step in the developmental history, which appears complete without it; and we can only record the facts, and hope that some more fortunate workers may be able to interpret them.

In the course of our work on this and other forms we have been more than ever strongly impressed with the danger of hasty conclusions. It animates our diaries to comment from time to time on the probable meaning of certain observed phenomena—to speculate on their relation to what we had fully ascertained and what we had yet to discover. At times, indeed, our inferences, *when*

* 'M. M. J.,' vol. ix., pp. 206–209.

made, seemed inevitable. But nothing is more interesting to us than to see how facts slowly and unceasingly pursued and ascertained and collated, showed the inutility of our surmises. In investigations of this kind we are convinced that sequences must be made *by the facts themselves*. Inference, however plausible, may vitiate a whole train of observation; and, amongst other things, we are bound to perceive the liability there must be to infer heterogeny if observations be not long continued, and every transitional step in the process be not demonstrated with the severest accuracy.

To complete our work on this form we conducted a series of heating experiments in precisely the same way as before. It will suffice, therefore, to give the results of one series, which may be taken as typical.

Six slides were taken: a drop of the fluid put on and covered with a thin cover. This was carefully examined, and if found to contain what was needed was allowed slowly to evaporate. The whole selected six were next slowly heated up to 250° Fahr., kept at this heat for ten minutes, and then allowed slowly to cool. When cold, they were carefully remoistened with distilled water—the water flowing readily under the cover by capillarity—and they were again examined and reported upon.

Before they were put into the heating apparatus in each case it was discovered that the elements required were there.

On examination after heating, and immediately after fresh moistening, nothing was visible but a baked amorphous mass. Two hours after this no motion of any sort was visible, save in two, where, with $\frac{1}{25}$th, excessively minute points were seen to be in a state of activity, which was translatory and not Brownian.

Twelve hours after minute bodies—almost certainly known to us as the very earliest motile form of the monad above described after development from the germ—were seen in four of the fields. These in two of the instances were traced up to full-sized monads of the form and with the developmental history of the form at which we were working; whilst the other two on the second day had many of the same in full maturity and complete action. The other two were wanting in this form.

From this it is clear that whilst in one condition this monad can survive desiccation, in another—the true sexual-germ-state—it can survive a temperature of 250° Fahr.

We now heated another set of six under precisely similar conditions up to 300° Fahr. But in this whilst some forms with which we were acquainted survived by means of their germs, this form was *wholly destroyed*, and not the trace of one in any form could be discovered in any of the slides.

II.—*On some Microscopic Leaf Fungi from the Himalayas.*

By Joseph Fleming, M.D., F.R.C.S., Surgeon Army Medical Department.

(*Read before the* Royal Microscopical Society, *Nov.* 4, 1874.)

Plate LXXXVI.

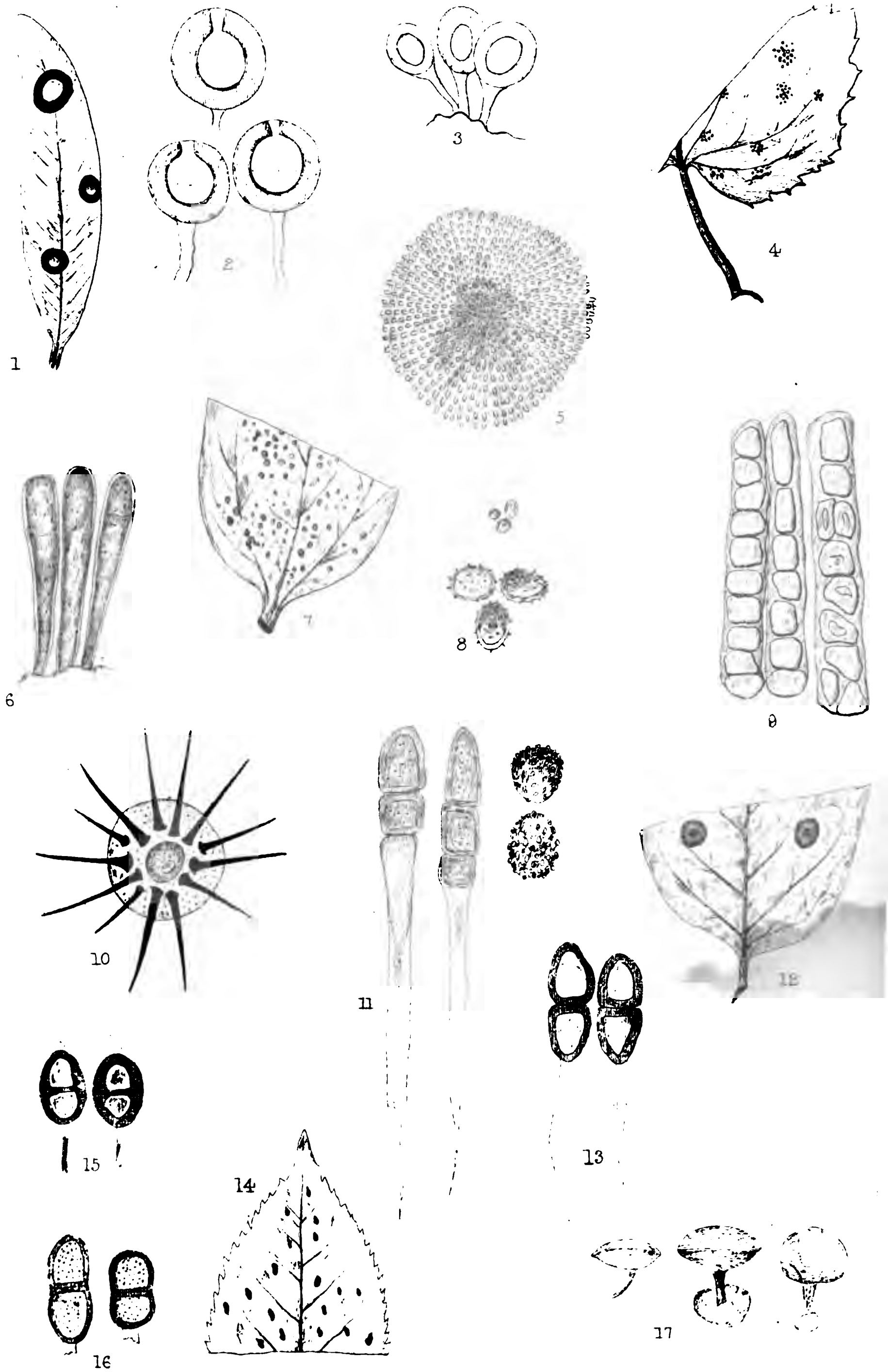
Some years ago, when the supposed fungoid origin of disease drew the attention and observation of many practical physicians and scientific men to that subject, more so perhaps than at the present time, I was led—no doubt as many other observers were—in the course of certain special investigations in India to make the acquaintance, though slight and for the first time, of that interesting and not generally known department of cryptogamic botany. Though not at any time successful in associating for certainty the presence of undoubted fungi with constitutional or epidemic disease, it frequently happened, as other observers both at home and abroad have noted, that certain forms were seen both in the human subject and in some of the lower animals while in perfect health.

However, in the present state of the question I would not pretend to affirm that many specific and constitutional diseases in man and animals are not caused by the presence of fungi or their sporidia in the body; and I cannot but believe that as our knowledge of them increases, and with the assistance of the higher magnifying powers, in conjunction with a careful co-relative analyses of symptoms, secretions, excretions, &c., we ultimately shall be able to say to what extent certain diseases may be owing to the lower forms of animal or vegetable life. I know the subject is a difficult one, and nowadays it must be confessed there are few men qualified for such investigations, simply because there are less material inducements than there ought to be, and hence men speculate and invent new theories, which are often more convenient and

---

DESCRIPTION OF PLATE LXXXVI.

Figs. 1 and 2.—*Uromyces ambiens*, on leaf of Himalayan box.
Fig. 3.—*Trichobasis* sp.
" 4.—Lower corner of leaf.
Figs. 5 and 6.—Probably *Uredo clematidis.*
Fig. 7.—Part of leaf, with *Uredo punctoidea.*
" 8.—Spores of *Uredo punctoidea.*
" 9.—Cells of *Septotrichum.*
" 10.—Probably *Volutella* or *Vermicularia.*
" 11.—Spores of *Coleosporium pingue* [?].
Figs. 12 and 13.—Part of leaf, with spores of *Puccinia dissiliens.*
" 14 and 15.—Part of leaf, with spores of *Puccinia cruciferarum.*
Fig. 16.—Spores of *P. umbelliferarum.*
" 17.—Erineum [deformed cells].



J. Fleming del.

W. West & Co. imp.

Himalayan Fungi.

satisfactory, rather than putting their shoulders to the wheel, and by experiment and observation trying to eliminate the truth.

It happened that for a period of two months in the autumn of 1870 I obtained leave to visit the Himalayas north of Deyrah Dhoon, and employed most of my time there in shooting game and large animals, especially bears, which are very plentiful in many parts of those truly magnificent mountains. In my search for sport, sometimes in places most difficult of access, I frequently observed fungi on the leaves or stems of living plants, and as opportunities permitted I made collections of the affected leaves, and preserved them for examination, which I regret to say has never been done until the present time.

During the space of four years many have changed their colour, and others cannot now be detected on the leaves, and some, indeed, have been lost; nevertheless I have endeavoured to collect a few, some of which no doubt are rare or even unknown in this country. I am not aware of the existence of any work on the fungi of the Himalayas, otherwise I should have consulted it prior to writing the present paper, and thus have saved myself from useless repetition and a little anxiety lest I should be imperfectly performing a labour which has already been well done by another. Since my return to England I have had the advantage of Mr. M. C. Cooke's opinion of the objects figured in Plate LXXXVI., which he has kindly named. Judging from what I observed in September and October, 1870, I should say that fungi are very common throughout the whole range of the Himalayas, and it is probable that all the English species, and maybe many new ones, are to be found with little trouble to any energetic and adventurous mycologist who may be fortunate enough to traverse those regions.

On some banks and sunny slopes of the hills the wild strawberry and raspberry are to be found in abundance, and on the under surface of the leaves of many will be noticed black spots, which on microscopical examination are proved to be the *Aregma* (Phragmidium) *obtusum* and *Aregma* (P.) *gracile* respectively, and differing in no way from the English species.

On the under surface of the leaves of the thistle (*Carduus lanceolatus*) the *Puccinia syngenesiarum* was frequently noticed, and so was the *Puccinia variabilis* on the upper and under surface of the leaves of the dandelion.

The blackberry brand, the *Aregma* (Phragmidium) *bulbosum*, was quite common on the under surfaces of the leaves of the blackberry bushes, and occasionally a white, tough, cotton-like fungus was met with, growing either from the under surface of the venation of the leaf or from the petiole, or both. This fungus will be referred to farther on.

The *Aregma* (Phragmidium) *mucronatum* was found on the

under surface of the leaves of the wild rose, and another form which was also common on oak leaves, laurels, and other species, and which will be referred to presently.

Peridermium pini was plentiful on the leaves of the silver fir and other pines, which grew in abundance in every direction, and a similar fungus was commonly noticed on the pine leaves about Dalhousie, more than a hundred miles from the hills north of Deyrah Dhoon.

The various grasses as well as the leaves of Mudwa (a species of millet) and the leaves of the rice plant, which is cultivated in many of the valleys, appeared to possess their share of fungi.

In the accompanying Plate the portions of leaves have been drawn the natural size from the actual specimens, and the spores are magnified about 320 diameters.

In a valley about twenty miles from the military station of Landour, between Deyrah Dhoon and Dunooltie, but close to and behind a high hill at the latter place, there is a considerable extent of forest, principally composed of a species of boxwood tree (*Buxus*), from 20 to 30 feet high, and with wide-spreading branches and with stems averaging from a few inches to 1½ feet in diameter. During my travels of many hundred miles through the Himalayas I never happened to come across a single specimen of box tree except in the place mentioned, and in which they appeared to grow well.

The under surface of most of the leaves contained a dark-brown fungus, resembling in outward appearance an Æcidium, varying in diameter from ⅛ to more than a ¼ of an inch, and frequently two or three of these orbicular patches may be found on one leaf.

The peridium bursts regularly in circular patches and the margins become irregular and recurved, exposing the dark-brown spherical spores. The spores appear to be peculiar and to belong to the genus *Uromyces*, having short peduncles, a thick cell-wall, and a light amber-coloured endochrome, which communicates externally by a funnel-shaped orifice on the side opposite the attachment of the peduncle, i. e. the upper part of the spore. This description will be better understood by referring to Fig. 1, which represents a box leaf with the fungus *in situ*, and Fig. 2, which shows three of the spores magnified. [*Uromyces ambiens*, Cooke.]

Fig. 3 represents spores from the under surface of the leaf of a plant which I fail to recognize, and which exhibits small light-brown spots made up of an aggregate collection of oval yellowish spores attached by short peduncles to the epidermis of the leaf (*Trichobasis*). The plant apparently belongs to the order Convolvulacea, is herbaceous, about 2 feet in height, and the stem, &c., contains a milky juice as well as the roots, which are used by the natives as a purgative. A species of clematis (probably the

Traveller's Joy) is common on the hills and in the valleys, and sometimes a beautiful orange-red fungus may be observed on the under surface of the leaves. This fungus on the fresh leaf emitted an odious smell—the smell of a recently killed bug—and was quite unbearable, but now both the red colour and the smell have disappeared. When the leaf is examined with a low power the spores are found to be arranged in little button-like clusters (Fig. 5) firmly packed side by side and of yellow colour (Fig. 6), and growing from the cellular dermis of the leaf almost at right angles. [*Uredo clematidis,** Berk.]

Fig. 7 shows the appearance of the leaf of a species of mimosa with a fungus on the under surface, and Fig. 8 the spores with three sporidia above. [*Uredo punctoidea,* Cooke.]

I found also a leaf of an unknown shrub with rust spots irregularly distributed on the under surface. The spores are many-celled, irregular, elongated, yellowish, and easily separate as a yellowish-brown dust without any traces of mycelium, vide Fig. 9. [This is a *Septotrichum* formerly included with fungi, but now discarded as a diseased condition of the tissues.—M. C. C.]

On the leaf of a species of willow, I observed on the upper and under surface a number of black spots, which were easily scraped off. On microscopical examination, these black spots were found to be made up of a number of bodies like that depicted in Fig. 10. They consist of a circular, yellow-coloured matrix, with an interior dark-brown-coloured nucleus, made up of fine mycelium, and interspersed throughout the latter there are numerous small cells. Outside the central nucleus, and arranged in a radiating manner, there is a number of jet black spine-shaped processes, apparently very brittle, as they are easily broken by pressure under the covering glass. [No fruit: probably a *Volutella.*]

Fig. 12 represents the appearance of a portion of the under surface of a leaf resembling the *Rumex acetosa,* on which raised circular reddish-brown spots of fungi are observed. The spores (Puccinia) are shown in Fig. 13, magnified, the cells being nearly separate, and attached to long, tapering peduncles. [*Puccinia dissiliens,* Cooke.]

On the leaf of the common dock, which differs in no way from that seen in this country, the upper and lower surfaces were studded with rusty minute fungoid spots, which appear to belong to the order Æcidiacei. The spores are circular, tuberculated, and of a yellowish colour. [*Æcidium rubellum,* P.]

Fig. 14 represents the appearance of a piece of the under surface of the leaf of a cruciferous plant, very common in the

* On the leaf I find *Uredo clematidis,* but not the structure indicated in the drawing. If this is manifest in its fresh state, then the *Uredo* would be a *Coleosporium.*—[M. C. C.]

Himalayas, and which is frequently found covered with dark-brown spots of fungi. The spores are septate, with short peduncles (which are liable to be easily detached), oval in shape, and with a comparatively transparent, nipple-like projection on the upper part, vide Fig. 15. [*Puccinia cruciferarum*, Cooke.]

Fig. 16 shows the appearance of the spores of a Puccinia found on the upper and lower surfaces of the leaves of an umbelliferous plant. [*Puccinia umbelliferarum*, D. C.]

Fig. 17 represents the magnified appearance of bodies which form patches on the under surfaces of the leaves of the oak, species of laurel, wild rose, &c. They resemble the agarics in shape, are hollow, or filled with a pink fluid, which, when pressure of the covering glass is used, exudes through the lower part of the stipe or stalk. [An *Erineum*, not a fungus, but diseased tissue.—M. C. C.]

Fig. 11 illustrates two of the sporangia which form the tough, cotton-like fungus met with on the under surface of the briar leaves, or on the petiole. The conceptacles which contain the spores are bi- and tri-partite, and terminate in long filiform appendages. Two of the nearly colourless tuberculated spores are depicted to the right of the sporangia. [I have seen no specimen of this. The figures might belong to *Coleosporium pingue*, but the description is rather that of some *Erineum*.—M. C. C.]

I feel that an apology is necessary for writing on a subject with which I am so little acquainted; but as I had collected the materials many years ago, I considered it better that they should be made public at once, rather than be lost. Perhaps others more competent may be induced to pursue the subject more fully than I have been able to do, as it is one of great interest, and not without some advantage, and indeed, at all events, the small collection of leaves now before me recalls to my memory many pleasant days spent amidst most beautiful scenery in one of the finest climates of the world.*

* The technical descriptions of such of the foregoing fungi as are new to science are published in the current number of 'Grevillea.'

NETLEY, *September*, 1874.

III.—*The Sphæraphides in British Urticaceæ and in Leonurus.*

By Professor GEORGE GULLIVER, F.R.S.

THE sphæraphides in the leaf-blade of some Urticaceæ are well known, and have been described on the Continent as "crystal glands" and "cystoliths." But there is another kind of sphæraphides in these plants which has hitherto escaped notice; and though Mr. Roper, in a late number of 'Science Gossip,' has made some good observations on the sphæraphides in the leaf of the wall-pelletory, the chemical composition of the two kinds of them in all the British Urticaceæ requires investigation. Nor have we yet any description of such crystals in Leonurus and other Labiatæ.

In *Urtica dioica, U. urens*, and *Parietaria diffusa*, the leaf-blades are studded with sphæraphides, each about $\frac{1}{532}$nd of an inch in diameter, globose, smoothish or granular on the surface, and all composed mainly of carbonate of lime. In the fibro-vascular bundles of the leaf are chains of much smaller sphæraphides, each about $\frac{1}{1000}$th of an inch in diameter, rough from projecting crystalline points on the surface, and composed of oxalate of lime; and in the pith these small rough sphæraphides are still more abundant. Both the leaf and pith of *Humulus lupulus* abound in like manner with the two kinds of sphæraphides. In the leaf-blade these are crystalline concretions, made up of glassy granules, consisting of carbonate of lime. In the leaf-nerves, and in the pith of the stem, are thickly-set strings of rough sphæraphides, in shape and chemical composition like those in the same parts of the nettles and pelletory.

The sphæraphides in *Leonurus cardiaca* seem to have escaped notice, though they are very distinct. In other Labiatæ I have not yet met with similar crystals, much less raphides. The leaf-blade of this plant is thickly dotted with globose granules, each about $\frac{1}{250}$th of an inch in diameter, rounded in form, and contained in a closely fitting cell, which is often tipped with a short unicellular hair; the globose crystalline matter consists chiefly of carbonate of lime. In the fibro-vascular bundles of the leaf are a few much smaller rough sphæraphides, each about $\frac{1}{2800}$th of an inch in diameter, and composed of carbonate of lime; but the pith of this plant contains no sphæraphides.

In all these examples the value of boiling the parts in a solution of caustic potass is so great, that the crystals are exposed more clearly than they appear before such treatment; and sometimes, when not easily found at first, the potass discloses them admirably, as may be proved in the leaf of *Ficus carica*. Though this species is so

nearly allied to the hop and nettles, the crystals in its leaf-blade are composed chiefly of oxalate of lime, without any appreciable trace of the carbonate, and the pith is devoid of sphæraphides.

Some practical applications of these facts appear obvious. They afford good examples of two kinds of crystalline concretions, differing as well in form as in chemical composition, existing in one and the same plant; that one use of the pith may be as a repository and laboratory of saline crystals, ready to be restored as manure to the soil; and that nettles and hop-bines should be thus utilized. Besides, the sphæraphides are so very beautiful, so easily examined and preserved, as to afford an abundance of excellent materials, ever at hand, for the employment of the microscope, and for preparations to enrich the microscopic cabinet.

CANTERBURY, *Nov.* 12, 1874.

IV.—*The Encystment of Bucephalus Haimeanus.*

By M. ALF. GIARD.

VON BAER pointed out long ago (1826) a peculiar parasite of the *Anodon*, which he called *Bucephalus polymorphus*. This parasite was later more fully described by Steenstrup and by Siebold, who gave it its true place in classification.

In 1854 M. Lacaze-Duthiers made known another species of the same genus, the *Bucephalus Haimeanus*, which he had found in the Mediterranean, and which lived parasitic in the genital glands of the oyster (*Ostrea edulis*), and also on the cockle (*Cardium rusticum*), which it renders sterile. The *sporocysts* and the *cercaria* form of this Trematode have been very carefully figured in an excellent paper in the 'Annales des Sciences Naturelles.'

Claparède has since found this curious Trematode at Saint-Vaast-la-Hogue, on the coast of Normandy.* It was in fishing in the open sea with a very fine meshed net that he caught the *Bucephalus* very frequently. The specimens drawn by Claparède differ a little from those represented by M. Lacaze-Duthiers; but this difference, which bears principally on the form of the lamellar appendages, did not appear important enough to the Genevese savant to necessitate the creation of a new species. Claparède no more than his predecessor has been able, in spite of his active researches, to succeed in making known the final destiny of *Cercaria Haimeana.*

* Claparède, Beobachtungen über Anatomie u. s. w. an der Küzte von Normandie, 1863.

The Bucephalus of Haime is to be found both at *Etaples* and in the neighbourhood of Boulogne-sur-Mer. Guided by certain theoretic views, the result of researches followed out upon the parasitic crustacea, I have been more fortunate than my two skilful predecessors, for I have proved the encystment of the Bucephalus.

It is upon the garfish (*Belone vulgaris*) that I have made this observation. This fish (at Boulogne the *maquereau d'été*, at Abbeville the *bécassine de mer*) occurs commonly in the market of Boulogne during the months of May, June, and the beginning of July. The viscera of this fish, especially the liver, the genital glands, and the peritoneum are frequently filled with minute cysts having the form of cylinders, terminated at one of their extremities by a bulb lightly drawn to a point, like a thermometer in construction. By opening cautiously a certain number of these cysts, one finds in some one of them the Bucephalus as yet untransformed.

My anatomical researches, interrupted in July, were not pushed so far as I should desire. However, I should say, in accordance with Claparède, that it is impossible to arrive at the opinion of M. Lacaze-Duthiers, when he says, in speaking of Bucephalus, "One observes in it a general cavity, that may be considered a digestive cavity." The position of the openings and their physiological purposes appear to me to be worthy of being studied anew.

What becomes of the encysted Bucephalus? Does it arrive at maturity in the body of the garfish, or does it undergo another migration? In this latter case, which is the more probable, is this migration active or purely passive? This it is which remains for discovery. Claparède has many times found the *Cercaria Haimeana* fixed upon the *Sarsia* and the *Oceania*. On one occasion the cercaria had lost its two long appendages, but it still wanted the reproductive organs. Claparède concludes that this fact was accidental, and that the medusæ were but momentary hosts of the Bucephalus. I myself have met an adult Trematode in the cœlenteric cavity of *Cydippe pileus*, which in spring is sometimes cast up in abundance on the shore of Wimereux: but I have no serious reason for supposing a genetic relation between this Trematode and *Bucephalus Haimeanus.*

According to Siebold, *Bucephalus polymorphus* is transformed into *Gasterostomum fimbriatum* in the digestive tube of *Perca fluviatilis* and *P. lucioperca*. We also find it encysted in the Cyprinidæ. It seems, then, very probable to suppose that *Bucephalus Haimeanus*, encysted in *Belone vulgaris*, is metamorphosed into a species of the genus *Gasterostomum* in the intestine of some large fish which the garfish is the prey of. In fact, Lacépède assures us that when the garfish quits the deep waters to go to spawn near the shore it often becomes the prey of sharks and large species of

Gadus, or other voracious and well-armed fish. Finally, as one has met Bucephalus in the liver of Paludinæ and Gasterostomæ, in the intestines of the pike, the eel, and of other fish, and even in the duck, I am compelled to think that the fresh-water species of this group of Trematodes are more numerous than is at present believed. The differences already pointed out between the marine Bucephalus of the ocean and that of the Mediterranean Sea will probably have a greater importance when we shall have made a more complete and comparative study of these animals.—*Comptes Rendus*, p. 485, August 17, 1874.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Decomposition of Eggs.—In a paper lately read before the British Association at Belfast, Mr. William Thomson said that researches on this subject were commenced by the late Dr. Crace Calvert and himself about the beginning of October, 1870, and extended over the following year and a half. From numerous experiments he drew the conclusion that whole eggs could only be attacked by one, two, or all, of three different agencies of decomposition. The first, which he termed putrid cell, is capable of being developed within some eggs, no matter how effectually their shells be protected by varnished coverings from the spores floating in the atmosphere. It is generated from the yolk. In some cases the yolk begins to swell and absorbs most of the white; in others the yolk bursts, and its whole substance becomes thoroughly mixed up with the white; and in others again it begins to change slightly, and then gives off minute cells into the white, rendering the white turbid; but in all cases where this takes thorough hold of the contents of the egg, true putrefaction commences, and the albumen emits a putrid smell. The minute granules or cells of the healthy yolk, when this decomposition commences, assume a morbid vitality; they grow large, and become filled with small cells; each large cell then bursts, and the smaller cells take independent existence. These cells are the bioplasm of the yolk, which, had the egg developed into a chicken, would have gone to form its flesh, bone, and tissues. These cells, under their morbid vitality, absorb oxygen, and liberate carbonic acid gas. Two eggs had their shells well varnished over with shellac, and were set aside on a shelf for one year, and both then opened. One appeared as fresh as on the day when it was set aside, but when the other was struck with the point of a knife to open it, the pressure of gas contained within the shell burst out, and scattered part of its contents in all directions. The next germ of decomposition—the vibrio—appears under the microscope like a small rigid worm which swims about. These animalcules are constantly found floating about in the atmosphere, but never penetrate into the contents of an egg if the shell be kept dry, but if the shell be moistened or wet, the dried bodies of these animalcules develop in that water, assume much vitality, and then penetrate the shell and set up putrefaction. Eggs were placed in fluids swarming with different animalcules, some like corkscrews, which swam by quickly turning round; others which appeared under the microscope like flukes, but which really had the form of an egg; some with one, some with two feelers, which swam by switching those feelers into a quick serpentine motion in front of them. These, however, were not able to penetrate the shell of the egg. The third is the fungus decomposition. The spores of this fungus are found everywhere floating in the atmosphere. They settle on the shells of eggs placed in stagnant atmospheres, and send myriads of filaments through the shell in all directions, sometimes binding all sides of the shell together, in all

cases converting the white into the consistency of a strong jelly, and often the filaments grow in such immense numbers as to make the whole contents appear like a hard-boiled egg. This fungus acts on the air exactly like animalcules, absorbing oxygen and liberating carbonic acid gas.—*The Medical Record*, Sept. 9th.

Effects of Section of Motor Nerves on Muscle.—Bizzozero and Golgi* give an account of an experiment they made upon a rabbit when six months old. On the 10th January they cut out a considerable piece of the sciatic nerve. The tibio-tarsal joint of the side operated on became thickened, and an ulcer formed upon the surface in the course of a month. The lymphatic glands swelled. The animal, however, retained tolerable health till the 20th August, when a portion of the crural nerve was excised on the same side. The rabbit remained pretty well till the 9th November, when the ulcer began to increase and assume an unhealthy aspect, and in December it died, having previously become exceedingly thin. On *post-mortem* examination the connective tissue of the whole lower extremity was found to be infiltrated with serum, ulcers had formed at various points, and beneath these were cheesy deposits, each of which was surrounded by a dense capsule of connective tissue. The stumps of the divided nerves were separated from each other by a considerable interval. The superficial muscles of the thigh presented a pale-rose colour, whilst the deeper ones were of a yellowish red. The superficial muscles of the lower leg were in general greyish red, but in parts yellowish; they felt hard and were easily torn. The deep muscles had undergone some thickening, and had a uniform yellowish colour; on section they appeared smooth and uniform, the surface of the section resembling bacon-fat. Microscopical examination showed that in the superficial muscles of the thigh there were scattered groups of fat-cells arranged in linear series, which apparently corresponded with the course of the nerve-fibres. The muscular fibres of the deep muscles of the thigh were thinned, the transverse *striæ* scarcely visible, and between the muscular fasciculi of the first and second order were numerous and very well defined fat-cells. In other parts the muscular substance of the several fibres was partly torn into fragments and partly replaced by fat-cells. The superficial muscles of the lower leg presented in a very marked manner the usual consequences of nerve section—namely, proliferation of nuclei in the muscle-corpuscles, atrophy of the muscular fibres themselves, increase of the interstitial connective tissue, and numerous fat-cells between the muscular fibres. Lastly, in the deep muscles of the lower extremity, where the muscles were yellowish and like bacon on section, no trace of muscular fibre was visible; the tissue appeared to be altogether converted into adipose tissue, comparable to the panniculus adiposus. In transverse sections the fat-cells were rounded or polyhedric, and formed a kind of mosaic. In longitudinal sections they were serially arranged in correspondence with the direction of the fasciculi.—*The Lancet*, Aug. 30th.

* Stricker's 'Jahrb.,' 1873, Heft 1.

*The Original Distinction of the Testicle and Ovary.**—This paper is of the utmost importance, as it bears so thoroughly upon Haeckel's theory. It is by M. Van Beneden, of Liège, and we quote the following translation of part of it from the 'American Naturalist' of November, 1874:—

"Huxley was the first who demonstrated that the entire organization of the zoophytes, medusæ, and polypes, hydroids and Siphonophores, can be reduced to a sac formed of two adjacent cellular layers, the ectoderm and endoderm (Allman), and who considered this proposition as expressing the general law of structure in the zoophytes.† Although one did not dream at this period of seeking homologies between the vertebrates and lower animals, Huxley took in all the bearings of his discovery. He recognized and formulated in clear and precise language his opinion on the homology which he believed exists between the ectoderm and endoderm of the Cœlenterata, and the two primordial cellular layers of vertebrates. See in what terms he expresses this idea:—'The peculiarity in the structure of the body-walls of the Hydrozoa, to which I have just referred, possesses a singular interest in its bearings upon the truth that there is a certain similarity between the adult state of the lower animals and the embryonic conditions of higher organizations.

"'For it is well known that, in a very early state, the germ, even of the highest animals, is a more or less complete sac, whose thin wall is divisible into two membranes, an inner and an outer; the latter, turned toward the external world; the former, in relation with the nutritive liquid, the yolk. . . . . The various organs are produced by a process of budding from one, or other, or both of these primary layers of the germ.'

"He seeks likewise to establish a parallelism, from a histological point of view, between the ectoderm of zoophytes and the external layer of the embryo of vertebrates on one hand, and the endoderm and internal layer on the other. He concludes by saying, 'thus there is a very real and genuine analogy between the adult Hydrozoon and the embryonic vertebrate animal.' All the embryological researches made in late years, in the first phases of the embryonic development of animals of all branches, have tended to confirm, extending it to the whole animal kingdom, the opinion of the illustrious English naturalist. And in the first rank of work done in this direction may, without fear of contradiction, be cited that of Kowalevsky; in showing the identity of development of Amphioxus and of the Ascidians, he closed with a single stroke the abyss, thought to be impassable, which separates the branch of vertebrates from all the lower organisms. The important publications of the same author on the other types of organization, added to those of Gegenbaur, Haeckel, Ray Lankester,

* 'De la Distinction originelle du Testicule et de l'Ovaire; Caractère sexuel des deux Feuillets primordiaux de l'Embryon; Hermaphrodisme morphologique de toute Individualité animale; Essai d'une Théorie de la Fécondation.' Bruxelles, 1874. 8vo, pp. 68.

† "Observations upon the Anatomy of the Diphydæ and the Unity of Organization of the Diphydæ and Siphonophoræ." 'Proceedings of Royal Society,' 1849.

Kleinenberg, and some others, have resulted in extending to the entire animal kingdom this grand conception that all the parts of the animal organism are formed from the two primordial cellular layers, and everywhere homologous.

"These ideas have just been developed in detail and brilliantly defended in two essays of a high philosophic import. Haeckel has proposed in his brochure 'Die Gastræa theorie, die phylogenetische Classification des Thierreiches und die Homologie der Keimblätter,' a theory which he had first announced in his monogragh on the calcareous sponges. Some analogous ideas, and in several respects almost identical, have been published in England in the Annals and Magazine of Natural History, under the title, "On the Primitive Cell-layers of the Embryo as the Basis of the Genealogical Classification of Animals," by my friend E. Ray Lankester.

"All the pluricellular animals, in which the development begins by the segmentation of the cell-egg, pass through in the course of their evolution a similar embryonic form, that of a sac whose thin walls are constituted of two adjacent layers, the endoderm and ectoderm. The first surrounds a cavity which is the primordial digestive tube; the second limits exteriorly the body of the embryo; it alone can be impressed by external causes. The digestive cavity communicates with the exterior by a single orifice which serves both as mouth and anus. The embryo is reduced to a digestive cavity, which is but a simple stomach; Haeckel has proposed to give to this primordial form the name of *Gastrula.* As this embryonic form occurs in the vertebrates as well as the mollusks, arthropods, echinoderms, worms and polypes, it is clear that the ectoderm is homologous in the different types of organization; that the endoderm has in all the same morphological value; that the primordial digestive cavity of vertebrates and that of all other types of organization have the same anatomical signification. The existence of this common form in the course of evolution of all the metazoal animals allows us to refer them to a common source; there is a convergence of the great types of organization and not a parallelism as had been urged by Cuvier and Von Baer. Finally, we can infer the existence at a geological epoch far back, of organisms like the Gastrula form; these organisms, probably varied in a thousand ways in their form and in their external characters, have been the common source of vertebrates, arthropods, mollusks, echinoderms, worms, and zoophytes; they constitute the very numerous group of Gastræades (Haeckel). If the endoderm and ectoderm are homologous in all the Metazoa [i.e. all animals except Protozoa], we then have a right to suppose that these two cellular layers have in all the same histological value, and that the same systems of organs are developed in the different types of organization from the same primitive layers. This induction has been already freely confirmed in that which concerns the central nervous system, which is developed in all animals from the ectoderm.

"Consequently, it makes no difference if we should wish to know the origin of an organ, whether we seek for it in one or another type

of organization; the results can be extended to the whole animal kingdom, and receive a general signification.

"However, of all the types of organization, that which serves best for research on this capital question of the origin of organic systems, is that of the polypes, still called zoophytes or Cœlenterates. In them, in short, the ectoderm and endoderm persist with their embryonic characters during their entire life; all the organs of the zoophytes are only a dependence of one or the other of these layers, sometimes of the two layers united.

"The polype form may be traced back with the greatest facility to the Gastrula, all the parts of which are preserved without undergoing any great modifications during all the course of existence.

"*Conclusions.*—In the Hydractiniæ, 1. The eggs are developed exclusively from the epithelial cellules of the endoderm. They remain, up to the time of their maturity, surrounded by the elements of the endoderm.

"2. The testicles and spermatozoa are developed from the ectoderm; this organ results from the progressive transformation of a primitive cellular fold formed by invagination.

"3. There exists in the female sporosacs a rudiment of the testicular organ; in the male sporosacs a rudiment of an ovary. The sporosacs are then morphologically hermaphrodites. . . . . Fecundation consists in the union of an egg, a product of the endoderm, with a certain number of spermatozoa, products of the ectoderm. This act has no other end than to unite chemical elements of opposite polarity, which after having been united an instant in the egg, separate again; for in most animals those in which the division of the vitellus into two occurs, the elements from which the ectoderm are formed are already separated from those which are to form the internal layer of the embryo.

"The new individuality is realized at the instant when the union between the elements of opposed polarity has taken place, as absolutely as a molecule of water is formed by the union of atoms of hydrogen and oxygen."

The Origin of Typhoid Fever.—In the beginning of the month of November a very important letter of some length on this subject appeared, from the pen of Dr. Tyndall, F.R.S., in 'The Times' newspaper. We would commend the letter to the attention of those of our readers who are interested in the subject. We differ from one of our contemporaries in the view we take of this letter; for, unquestionably, although its facts are not novel to the scientifically educated medical man, still, to the mass of surgeons, and to the whole of the non-medical community, it is absolutely and completely novel. And it seems to us that a certain amount of credit is due to Professor Tyndall for thus manfully coming forward to discuss, in a purely popular form, facts which he, of course, knew were well enough known to certain professional minds. He has at once spread throughout the country views which, had he not come forward, might have remained where they had been till the next half century.

The South African Diamonds.—Mr. G. C. Cooper, who may be said to be "to the manner born," as he comes from the African diamond-fields, has an interesting paper in the last volume of the 'Proceedings of the Geologists' Association' (Oct., 1874), on this subject. Besides other matters of non-microscopical interest, he says:—"I have recently received a piece of what is termed 'chalk' by the diggers, which, from microscopic observations, I would infer to be in all essential points the same as the tufa that is so abundant, with this exception, that this has been broken up, disintegrated, as on a beach, and thus made into a softer form, after its deposition as tufa. It gives the same white streak as ordinary chalk. It has imbedded in its surface some sand granules, and black crystals, resembling, and, as I think, the same as occurs in a sample of stuff lately given to me, from 80 feet depth. Its component particles are crystalline and translucent, and from $\frac{1}{2000}$ to $\frac{1}{4000}$ of an inch in diameter. Dissolves in dilute muriatic acid, with some flocculent deposit. Upon evaporation, star-like masses of crystals form. I look upon this specimen as of importance in helping to substantiate what I advance in relation to water and ice in producing the diamond deposit."

The Structure of the Testicle.—A good paper on this subject is that done by Dr. Victor v. Mihalkovics, and the results of his researches appear in the last part of the 'Arbeiten,' or "work done" in the Physiology Laboratory of Ludwig, at Leipzic, in 1873. The conclusions at which Dr. Mihalkovics has arrived, which partly agree with and in part differ from those of previous observers, are given in the following manner by the 'Lancet' (Oct. 10):—In the first place, he finds, in opposition to the greater number of authors, as Müller, Krause, Beale, Sappey, Kölliker, v. Luschka, and Lavalette St. George, that the tortuous terminal or peripheric portion of the tubuli seminiferi forms a plexus by the anastomoses of their numerous dichotonous divisions. The ultimate branches appear to be connected by loops. In man, the canals in the cortical layer present small bead-like projections of the wall, and never begin, as most of the above-named authors contend, by closed free extremities. In regard to the structure of the walls, he believes Henle is most exact in stating that it consists of a series of laminæ, or membranes with flat nuclei. The size of the tubuli bears no relation to that of the testis, since in the guinea-pig it is 0·10; the cat, 0·11; in the cock, 0·12; mouse, 0·15; rabbit, 0·20; goat, 0·20; man, 0·21; dog, 0·25; bull, 0·26; and rat, which is the largest of all, 0·40 of a millimètre. Secondly, the straight portions of the tubes, or vasa recta, are not direct continuations of the tortuous portions, but are of very much smaller diameter, and are lined by a much shorter columnar epithelium. They run in the connective tissue of the corpus Highmorianum or in the lowest parts of the septa. Thirdly, the supporting cells, described by v. Merkel, and germ plexus are, he thinks, artificial products, which owe their existence to the coagulation of a tenacious albuminous substance occupying the interspaces between the seminal cells. Fourthly, certain interstitial cells are constituents of the testis, the analogues of which are discoverable in many other organs, as the supra-renal capsules, the

sacral and carotidean glands, the corpus luteum, and pituitary body. Fifthly, the connective tissue of the testis consists of various-sized trabeculæ of connective tissue, which form a network, and are covered by an endothelial layer of cells, which last is continued from them on to the seminal tubules and blood-vessels. Sixthly, the lymphatics commence in the interspaces of the fasciculi of connective tissue invested by endothelium, and partly in the lacunæ of the lamella of the walls of the seminal tubules. No true tubular lymphatics with defined walls exist in the testis at all. Lastly, the tubuli seminiferi are closely surrounded by a layer of capillary blood-vessels, intimately connected with the membrana propria.

NOTES AND MEMORANDA.

Cell-culture in the Study of Fungi.—Ph. Van Tieghem and G. LeMonnier in their published researches on the Mucorini give a good working account of their method of cell-culture which is applicable not only to the smaller fungi but to many other plants. The method is as follows, according to the 'American Naturalist,' Nov., 1874:—A glass cell $\frac{1}{8}$ or $\frac{1}{6}$ inch is cemented upon a glass slide, and a suitable cover glass is kept in place by three minute drops of oil placed on the edge of the ring. The contained air is kept moist by a few drops of water placed in the bottom of the cell, while a very small drop of the nutritive fluid is placed on the lower surface of the cover glass, and in this drop the spore to be cultivated is sown. The whole drop, and indeed the entire contents of the cell, can now be examined with suitable powers, and the germination and development of the plant traced hour after hour from any given spore, with the greatest certainty and ease. Extraneous spores will sometimes be introduced, but they are easily detected.

American Opinion on Angular Apertures.—An anonymous writer in the last number of the 'American Naturalist' makes the following remarks on this subject:—"It is not yet forgotten that at the London examination of the $\frac{1}{8}$-inch lens sent to demonstrate the possibility of obtaining an excessive angular aperture in immersion work on balsam objects, the lens was measured at an adjustment of which nothing to the point was known except that it was not a position of immersion work at all, nor a recognized maximum position for any kind of work; the plain fact being that the accomplished committee were so bent upon teaching us the familiar fact of reduced angle that they seem to have forgotten to look for any other possibility in the case. Nor is it likely to be forgotten as long as Mr. Wenham so far forgets his usual and admirable caution as to allude to the correction of this palpable mistake as an 'after quibble,' nor while the eminent President of the Royal Microscopical Society utters in his formal Address such an

astounding statement as the following:—'The lens in this instance was properly corrected as a dry lens, and then after measurement in air it was measured in water and then in very fluid Canada balsam without alteration of the adjustment. It may be quite possible that if the lens had been readjusted so as to give the best image for immersion in balsam, a slightly greater angle might have been obtained; but this would not have been a fair way of making a comparison, as it is not the mode in which the glass would ever be employed in actual practice.' By not saying squarely, *It is probably true that if the lens had been readjusted so as to give the best image for immersion in water, a greater angle would have been obtained; and this would have been the fair way of making the measurement, as it is the mode in which the glass would be employed in actual practice*, Mr. Brooke lost a rare opportunity to do a noble if not a generous act. As he is well known to be incapable of an intentional sophistry which by adroitly worded phrase should suggest a doubt where none is felt, belittle the concessions which are called for by manifest truth, and say one thing which is true but has no relation to the case at issue, and at the same time imply another thing which does not relate to the case but is unqualifiedly incorrect, there is no choice but to conclude that his extraordinary statement, notwithstanding its tone of judicial coolness, was made without that deliberation which the official character of the Address demanded.

"On the other hand, a still more recent lens by the same maker, claiming still more excessive aperture, has been examined by Mr. Wenham by his method of cutting off false light. By this method, which would seem incapable of excluding any image-forming rays, he succeeded in obtaining a clear and distinctly limited angle for the lens whose light, when not thus protected, was vague and uncertain; the angular aperture at the same time being reduced from '180°' to '112°,' which corresponded within a few degrees with the aperture computed trigonometrically from the width of the front lens and the length of the working focus. To this it is answered, that with a dry object on the cover there is *no* distance involved and the triangle is impracticable: while accurate focussing upon a stop which is feasible at 'uncovered' adjustment, is liable to error from spherical aberration when adjusted for maximum angle. Mr. Tolles' method of demonstrating the utilization of extra-limital rays is by placing a central stop upon the posterior surface of the back system of lenses, so large as to cut off all light when the objective is used dry; so that by no trick of illumination can the light be made to pass through the narrow ring of clear aperture remaining around the stop; but if water be flowed in both above and below the balsam-mounted object, converting both the objective and the illuminating semi-cylinder into immersion arrangements, a well lighted and defined image is immediately produced. With regard to extreme angles in connection with dry objects, Mr. Tolles claims that his much-disputed $\frac{1}{6}$ inch does actually form an image with the most oblique rays that can impinge upon the slide, all other rays being cut off by a card or shutter which can be moved up close to the bottom of the slide."

The Microscopic Structure of Ancient and Modern Volcanic Rocks is the title of a most valuable paper read before the meeting of the Geological Society, on Nov. 4th, by Mr. J. Clifton Ward, F.G.S. Unfortunately we have not space for a sufficiently long abstract in the present number, but we nevertheless call the attention of our readers to the subject. We shall give a full account of it in our next number.

How to Make exceedingly Thin Glass Covers.—The following exceedingly interesting paper we quote in full from the 'Quarterly Journal of the Quekett Club,' Oct. Mr. G. J. Burch, who is the author, says:—"Take a piece of glass tube of about $\frac{1}{4}$ inch bore, seal up the end with the blow-pipe, and continue the heat until the glass is so soft that it will fall out of shape, unless you keep turning it round; *remove it from the flame*, and blow into it with all your strength. It will be seen to swell, at first slowly, and then suddenly to a large bubble of very thin glass. Supposing the tube to have been sealed up with as little glass as possible, it may be blown out to about 4 inches diameter. When cold, break it up, and cut the pieces to shape with a 'writing diamond.' The glass in this state is of course convexo-concave; practically this is of little consequence unless the objects are to be mounted dry, when it is liable to be broken. In order to flatten it, place a piece of the thin glass on a perfectly flat piece of platinum foil, and depress it for a moment into the Bunsen flame; as soon as it is red hot, it will sink down to the flat foil. This also has the effect of annealing it. On measuring a piece of this glass with the micrometer, I found it to be $= \frac{1}{2500}$ inch $= \cdot 0004$ inch. In the 'Monthly Microscopical Journal,' vol. viii., page 270, Dr. Royston-Pigott says:—'The thinnest glass in my possession measures $2\frac{1}{5}$ thousandths.' Now $2\frac{1}{5}$ thousandths $= \cdot 0022$, and $\because \frac{0022}{0004} = 5 \cdot 5$. So that his thinnest glass is $5\frac{1}{2}$ times the thickness of mine."

CORRESPONDENCE.

Ross and Co.'s $\frac{1}{8}$th and Beneche's No. 7.

To the Editor of the 'Monthly Microscopical Journal.'

Minster Court, York, *November* 7, 1874.

Sir,—Until of late years England stood unquestionably at the head of all other nations in the production of object-glasses for the microscope, but now Paris, Vienna, Berlin, Munich, and Boston dispute the palm with London; and there is no denying that they are very formidable rivals.

There are, of course, great difficulties in the way of estimating the relative merits of objectives so long as they are handled by

different persons; differences in the skill and eyesight of the observers, differences in the modes of illumination, differences in the same nominal subject of examination, differences of angular aperture, and so on. These, it is plain, are more or less unavoidable; but there is one source of embarrassment, which, though it has often been mentioned before, can never be mentioned often enough, for evils are never remedied unless a loud outcry is raised against them. I allude to the total absence of any standard of magnifying power from which we are now suffering. I know of $\frac{1}{4}$ths which amplify much more than $\frac{1}{5}$ths, of $\frac{1}{8}$ths which exceed $\frac{1}{10}$ths, of $\frac{1}{12}$ths which are equivalent to $\frac{1}{15}$ths. In fact, matters have now come to such a pass, that an inexperienced purchaser can seldom know much more about what he is buying than that it is an object-glass. It would be a great boon to the world of microscopists if the Royal Microscopical Society could put forward a standard measure of linear dimensions for a given focus, and that our great makers would at least try to approximate to it; for the present system is an affront to common sense and common honesty.

But, notwithstanding these difficulties, comparison between English and foreign objectives is going on, slowly but surely, sometimes noisily, oftener silently, yet still going on, searching for facts, and awaiting a final verdict. As yet our country does not appear to have been worsted in the trial, although Mr. Mayall would probably think that the case has already gone against her, and that foreigners are of his mind.* There is one circumstance, however, that English opticians would do well to keep continually before them, that Europe has secured an immense advantage over her in the matter of price, and that nothing but quality can ever make head against cheapness.

As bearing upon this question, and with a full sense of the embarrassments which surround it, I would venture to make a few remarks in connection with the interesting note from Mr. Kitton in your last number.

As far as I am able to judge, I should say that Ross and Co.'s patent $\frac{1}{5}$th is about a match for his Beneche's No. 7 in magnifying power, though it may exceed it in angular aperture. On applying this objective to the examination of the diatoms which he has named, I found that with perfectly direct candle-light, mirror and diaphragm being both excluded, the B eye-piece revealed the striæ on *P. angulatum* most beautifully, and without the smallest change of the conditions brought out the arrangement of the terminal striæ perfectly well.

With the help of lamp and condenser, and using the C eye-piece, the checker-work of *P. intermedium* was exhibited most distinctly, and the costæ of *Cymbella Ehrenbergii* plainly seen to be composed of flattened beads.

Pinnularia peregrina is a more difficult object than *Cymbella Ehrenbergii*, but in one frustule the transverse lines upon the costæ were shown almost vividly; while those on *Nitzschia sigmoidea* stood out quite distinctly for all their closeness.

The transverse markings of *Synedra robusta* were distinctly re-

* See 'M. M. J.,' Feb. 1869, p. 90.

solved into beads, but apparently not so compressed as those of *Cymbella Ehrenbergii.*

In all the above observations the adjusting collar of the glass remained unaltered, just midway between "covered" and "uncovered." May I add that this objective contains in itself an adaptation for immersion use, and while it performs so well on lined objects, it gives a superb figure of *Lepidocyrtus curvicollis.*

I am, Sir, your faithful servant,

R. CORBET SINGLETON.

ON MR. SINGLETON'S OBSERVATIONS.

To the Editor of the 'Monthly Microscopical Journal.'

DENSTONE, *November* 9, 1874.

SIR,—While I am much obliged to the Rev. R. Singleton for his readiness to ventilate the question of straight candle-light illumination, I must confess I am disappointed at the general tone of his letter, and the covert vein of sarcasm which runs through it. This has been the more surprising to me, as I am conscious of having taken unusual pains to avoid giving offence, and to write nothing that might rouse up any of the *genus irritabile microscopicorum.*

Indeed, it would be well for all of us, when we have to remark upon the performances of another, of which we have before us only a brief printed account, to exercise a certain amount of caution, lest, while criticising, we ourselves fall into mistakes. With nothing but an abstract to guide us, and in the absence of the writer himself, we are not always certain what is the strength of the point we would attack; nor do we know but that our opponent may have an awkward trick of keeping back his strongest troops in the reserve.

Mr. Singleton must himself by this time regret the peculiar turn he gave to his last sentence.

"If he means that it has detected the longitudinal lines of that diatom, it would be a real boon to microscopists to tell them of the feat."

Indifferent persons who take up his letter will read between the lines something of this sort:—

"*S. gemma* is a test * of prodigious difficulty. Mr. Hickie seems to hint that he has resolved it with a $\frac{1}{4}$ inch. Either he has so resolved it, or he has not; he declines to say which. I will force

* The Germans, in spite of the *Gründlichkeit* we are in the habit of ascribing to them, are in these matters pretty much as we are ourselves, and quite as much given to copying one from another. See Dr. Hager's 'Das Mikroskop,' p. 36. An exception may be made in favour of Dr. E. Hartnack, of Potsdam, as his blunders are usually original. But the *Herrschaft zu Waisenstrasse*, though great opticians, are by no means great manipulators, as I know by experience. Something of the same kind appeared also in an early number of this Journal; but the writer has since, in my hearing, candidly retracted his error. To those who have no opportunity of judging otherwise, I would recommend a glance at Dr. Woodward's photograph of this diatom.

him to speak out plainly; and if he says he has, I am prepared beforehand to disbelieve him."

I have no right to assume—what certainly does not appear from Mr. Singleton's letter—that he has given any special attention to diatoms *as tests;* but those who have done so know well that, when once they have fully succeeded in mastering such a test with a moderate power—say a $\frac{1}{24}$ inch—a little practice and perseverance soon enable them to overcome it with a weaker objective, and that a little further practice generally resolves it with a weaker power still. Resolution turns not so much upon objectives as upon the manipulator's own fingers.

My use of the words "up to *S. gemma*" was an intentional concession to the frailty of human nature, in order not to offend the prejudices of those who persist in classing that diatom as a very high test.

In my own practice, I know only four first-class tests: (1) *Amphipleura pellucida,* (2) *Stauroneis spicula,* (3) *Navicula crassinervis,* (4) *Frustulia Saxonica,* as it is found in Eastern Prussia. In some of these latter, especially in those mounted by C. Rodig, of Hamburg, the lines are so amazingly fine, as to render this kind of Frustulia a more difficult test even than the *acus.* The second mentioned may be found on almost any slide of *P. macrum.* It is a small lanceolate species of *Stauroneis,* somewhat like two dagger-blades placed hilt to hilt. I recommend it to the notice of the readers of this Journal.

But with regard to *S. gemma* itself, I attach no sort of value to this diatom; and I have often wondered how such a thing ever got voted into a test.

Its deficiencies are obvious. It almost never presents an even surface; there is no uniformity in the same gathering in respect of difficulty, some specimens, especially those of a whitey-brown colour with almost black ribs, being troublesome enough, while many of those with a greenish tinge are scarcely a test for an ordinary $\frac{1}{8}$ inch. For instance, I have one such slide so easy of resolution, that I can resolve six out of every seven of those that lie perpendicularly, taking them just as they come. In some the interspaces between the ribs are twice or three times as wide at one end as they are at the other. In others, again, some of the ribs run only half-way, leaving a wide space; and though the longitudinal lines almost invariably, instead of forming straight vertical lines, slope somewhat from right to left, I have sometimes met with instances to the contrary.

In short, *S. gemma* seems made for the express purpose of upsetting all our theories as to the uniformity of structure and regularity of marking of Diatomaceæ, and, like many Christian men and women, is consistent only in its inconsistency. I think, therefore, I am justified in discarding it as a standard test.

Now, if I can produce satisfactory evidence that I have shown, clearly and distinctly, the longitudinal lines with an antiquated $\frac{1}{8}$ inch, it will, I suppose, render it somewhat probable that I might "detect" them with a remarkably good $\frac{1}{4}$ inch of latest date, especially if the latter have the advantage of illumination so helpful as,

according to Mr. Singleton, to render testing thereby "little better than child's play," to say nothing of the peculiar arrangement I used instead of an eye-piece, which Mr. S. somehow overlooks.

"SANDICROFT.

"I certify that I saw the longitudinal stripes on *Surirella gemma* with Mr. Hickie's $\frac{1}{8}$-inch objective, with the utmost distinctness. This was on the evening of the 23rd day of November, 1871.

"H. P. STEADMAN."

This gentleman could not possibly make any mistake about the lines he speaks of; for I had just previously let him see the very same lines on the very same shell with a $\frac{1}{40}$ immersion. I showed them also to Dr. Eales, of Dresden, who made a drawing of them then and there.

I am sorry I cannot in the same way quote chapter and verse for what I have seen with Beneche's No. 7; so the following must pass for what it is worth.

In a private letter to my friend, Mr. Kitton, of Norwich, in which I gave him a detailed account of the performance of Beneche's No. 7 on a variety of tests, the following passage occurs, which Mr. Kitton's kindness in returning me my letter has enabled me to extract:—

"I then put on *S. gemma*. I was able fairly to bring into view the longitudinal lines."

But after all, I am not the only person who has seen these longitudinal lines with a $\frac{1}{4}$ inch. Mr. Jabez Hogg, who is the fortunate possessor of a Beneche's No. 7 of rare excellence, has done the same with *his* glass; and did so in my presence, though I do not recollect the illumination he employed.

Unfortunately the microscope is a solitary instrument; and Mr. Brown finds it hard to believe that Mr. Smith has seen anything which he (Mr. Brown) cannot see. Hence come strife and debate. Readers of this Journal will be at no loss for instances. I have also to thank Mr. Singleton for his caution about "diffraction." In return let me caution him against a much more besetting sin of these times—"slowness of heart to believe."

Yours faithfully,

W. J. HICKIE.

REPORT OF QUEKETT MICROSCOPICAL CLUB, SEPT. 25TH.

To the Editor of the 'Monthly Microscopical Journal.'

GARRICK CHAMBERS, *November* 17, 1874.

SIR,—Will you kindly allow me to correct a slight inaccuracy that appeared in the November number containing the above report? I am there made to say I never found evidence of air in any insect's salivary glands. It should read—

"In a large number of insects examined he had never found any evidence of tracheal or air sacs forming part of their salivary glands."

I remain yours very truly,

WILLIAM T. LOY.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *November* 4, 1874.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A number of donations to the Society were announced, and the thanks of the meeting were voted to the donors.

The Secretary said that Mr. Suffolk, a Fellow of the Society, had presented them with an ingenious little apparatus, known as mechanical fingers, and which had been modelled after the plan of Professor Smith.

The President observed that it was for the purpose of picking up minute objects, such as diatoms, by means of a bristle held by some forceps. It was a very ingenious contrivance, and was worth the examination of the Fellows.

A vote of thanks to Mr. Suffolk was unanimously passed.

The President announced that the Council purposed holding another scientific evening on Wednesday, December 9; this arrangement was of course subject to the consent of the authorities of King's College,* and due notice would be sent to all the Fellows of the Society in the usual way.

The Secretary said they had received a paper "On Microscopical Leaf Fungi from the Himalayas," by Dr. Joseph Fleming. The paper and the drawings which accompanied it had been shown to Mr. Cooke, who had found amongst them a number of genera and species which were apparently identical with European species. The paper was one which he thought would be more interesting to Fellows when they could read it for themselves; he would therefore only mention the fungi discovered. It would be printed in full in the Journal, with the drawings and the remarks of Mr. Cooke upon them. The paper will be found at p. 270.

The thanks of the Society were voted to Dr. Fleming and Mr. Cooke.

The Secretary then read a paper by Dr. Drysdale and the Rev. W. H. Dallinger, entitled "Continued Researches into the Life History of the Monads." The paper was illustrated by numerous drawings, which were enlarged upon the black-board by Mr. Charles Stewart. The paper will be found at p. 261.

The President, in proposing a vote of thanks to the authors of the paper, remarked that there were several points in it which were

* Since obtained.

matters of great interest. The twofold mode of reproduction mentioned—fission and impregnation—was remarkable. One of the processes might prove to be the same as parthenogenesis, which was known to exist in the case of the aphis. Another great point was the bearing of some of the observations upon the important question of spontaneous generation; because if the germs alluded to had been found by experiment to survive after exposure to a temperature of 250° or 300° Fahr., it was quite clear to him that the observations of Dr. Bastian must be looked upon as wholly inconclusive.

Mr. H. J. Slack said he would just call attention to the excessive minuteness of some of these "moving points," as they were called in the paper; for if a skilled observer, using a power so high as $\frac{1}{50}$ inch, can only describe them as moving points, the actual objects themselves must be almost infinitely small, and it could only be from the difference of their refractive power that they could be seen at all. The impression given was, that if they were only a little smaller or were nearer in refractive index to that of the fluid in which they moved, though there might be myriads of them there, they would be utterly invisible. They also found from the paper that they must not conclude that even a high temperature would destroy life. They had usually supposed that the process of heating organisms produced similar effects to the coagulation of the albumen in a boiled hen's-egg; but it was probable that a proteine substance which was not changed in that way might survive any temperature which failed to actually disintegrate it, and in the case of a hydro-carbonaceous compound it could not be destroyed by anything short of actual burning. If carbon had only been known to them in its combustible forms it would have been received with much doubt that there might be conditions under which it was difficult to burn it, but they knew that a piece of graphite could not be burnt in a candle, or a diamond with a lucifer match. These things showed them how very careful they should be not to rely upon any merely negative evidence as to organisms and germs being destroyed by heat.

A vote of thanks to the authors of the paper was carried unanimously.

Mr. Charles Stewart called attention to some living organisms exhibited in the room by Mr. Wood, and which bore a very strong resemblance to the one shown there at the last meeting, and which he thought to be allied to *Bucephalus polymorphus*.

Mr. Wood said that one of the Fellows of the Society had mentioned to him that an object had been exhibited at the last meeting very much like the one which he now exhibited, and he had therefore endeavoured to bring his specimens there for inspection. The objects were, he believed, the larvæ of the cockle. He also exhibited some drawings taken from his note-book showing the other stages of its development. He at first had supposed that these organisms did not belong to the cockle at all, but further observation showed that they were really the larvæ, and he had traced them up through all their forms.

The thanks of the meeting were voted to Mr. Wood for his interesting communication.

Mr. Charles Stewart said he had been afforded an opportunity of looking at this supposed larva, and, so close was its resemblance to the *Bucephalus* exhibited at the last meeting, that he could not help thinking if that was an entozoon this must also be something of the same kind. It was so unlike the larval forms of the lamellibranchiate mollusca that he thought it might after all turn out to be really a parasite, and its position in the ovary would not negative this notion. The resemblance between the two was really so very close that he could not help thinking that the position of both in the animal kingdom would prove to be the same, though, of course, he did not say that it was not the young of the cockle. Mr. Stewart then drew upon the black-board the object exhibited by Mr. Badcock at the previous meeting, and also a copy of Mr. Wood's drawing of the one he had brought that evening, and pointed out the similarity between them.

Mr. Wood said that he had never found anything else than these creatures in the ovary of the cockle.

Mr. Stewart thought it might be worth while to institute some further comparisons between the two objects in their earlier stages; he did not say, of course, that the two were really the same thing, but he was quite disposed to bracket them together as being of the same genus.

Dr. Moore said that he had been for some time examining both the cockle and the mussel, and had traced out the development of the cockle in the same way. He believed that these objects were the larval forms of the cockle, and in the marine mussel he had also traced out the development. He found that these long arms consisted of striated muscular tissue.

Mr. Stewart said he had examined the arms under $\frac{4}{10}$, but had found no trace of striated muscle—it might, however, perhaps require a higher power.

The President observed that the fact of such analogous organisms having been found in so many instances as described would lead one to suppose that they might be the young of the cockle instead of Entozoa.

Dr. Moore inquired what proof there was that the *Bucephalus* was an entozoon.

Mr. Stewart said it was considered so by some who were thought to be authorities.

Dr. Moore said he had been able to trace the rudimentary shell both in the cockle and in the mussel.

Mr. Stewart mentioned that in the last number of the 'Annals of Natural History' there were a number of references to *Bucephalus*, and perhaps they might throw some light upon it. He must say also that when he first saw a drawing of *Bucephalus polymorphus* he fancied that it might possibly be the young of some lamellibranchiate, but did not think so after examining the creature itself.

Perrya pulcherrima (Kitton) and some other new species of diatoms were exhibited under one of the Society's instruments.

Donations to the Library and Cabinet since Oct. 7, 1874:—

Nature. Weekly	From	*The Editor.*
Athenæum. Weekly	„	*Ditto.*
Society of Arts Journal. Weekly	„	*Society.*
Journal of the Linnean Society. No. 77	„	*Ditto.*
Journal of the Quekett Club. No. 27	„	*Club.*
Bulletin de la Société Botanique de France	„	*Society.*
Marvels of Pond Life. By H. J. Slack. 2nd Edition	„	*Author.*
The Protoplasmic Theory of Life. By John Drysdale, M.D.	„	*Ditto.*
Mechanical Finger	„	*W. T. Suffolk, Esq.*
Four Slides of Diatoms	„	*F. Kitton, Esq.*

The following gentlemen were elected Fellows of the Society:— John Railton Williams, Esq.; James Wallinger Goodinge, Esq.

WALTER W. REEVES, *Assist.-Secretary.*

MEDICAL MICROSCOPICAL SOCIETY.

October 16, 1874.—Jabez Hogg, Esq., President, in the chair.

At the first meeting of this Society, for the session 1874–5, a paper communicated by John Gorham, Esq., of Tunbridge, "On a New and Expeditious Method of Micrometry," was read by the President.

The principle of the instrument described, depended upon the measurement of lines drawn parallel to the base of an isosceles triangle—the base of the latter being given—by means of the sides, which are divided into a known number of parts. The triangle is obtained by dividing through the centre a disk of brass, about 1¼ inch in diameter and half an inch thick, and bevelled at the edge, so as to allow of its being embraced by a stout india-rubber ring, by which means the two portions are held in perfect apposition at the edges of the section. The line of section, for the distance of 1 inch from the circumference, is marked out into fractions of an inch—at least, into 32 parts—a less number being insufficient to obtain accurate results. A piece of paper of known thickness is now inserted between the halves of the disk, and moved along till its edge touches the commencement of the marked inch, the elastic band retaining it in its place, and thus an isosceles triangle, or gap, is left, with a base the thickness of the slip of paper, and with an edge of 1 inch, divided, as stated, into 32 equal parts. If a hair or cobweb be passed along the slit from base to apex, it will be arrested somewhere, and by reading off the number opposite which it stops, a simple matter of multiplication, the base of the triangle being known, will give the diameter required. For microscopic purposes the instrument is placed on the stage, and the object to be measured, placed on a thin glass cover, is slid over the aperture till it exactly at one point spans it. The diameter is then read off. To obtain still greater accuracy, Mr. Browning has added a screw of known value, to separate the halves of the micrometer in lieu of the slip of paper.

In answer to some questions by members of the Society, the President replied that the instrument was specially designed for unmounted objects, the thickness of an ordinary glass slide being rather an objection in the case of mounted ones. A thin glass cover might be in all cases employed for placing the specimen, e. g. blood, or pus, upon.

READING MICROSCOPICAL SOCIETY.*

October 13.—Captain Lang exhibited mounts of webs of three kinds of spider; the first that of *Epeira diadema;* the second of an undetermined species, as unfortunately he could never find the owner at home (though the web was renewed each night, after being destroyed); and the third also unknown, being an old bought slide.

All three were furnished with the viscid beads so well known on the concentric threads of *Epeira;* but those on the third slide were much larger, and the threads appeared to be not spiral or concentric; whilst the arrangement of beads on the second was very different and peculiar. This web was not geometrical.

Till lately it was generally considered that the spider, after forming its web, went over it again, adding the viscid drops or beads; but the late Richard Beck exploded that fallacy, by simply watching (under a microscope) an *Epeira* making its web, when he saw that the thread, after emission, ran into beads by molecular attraction.

Captain Lang, however, though accepting this general fact, considers that two of the three pairs of spinnerets are employed in the formation of the thread; the simple line issuing from one pair whilst the other pair varnishes it with a viscid secretion running into beads by molecular attraction, as saliva will on a hair passed between the lips. The second slide seemed to prove this; for, whereas in the *Epeira* web the beads were seen to be arranged *singly* along the line, and might, therefore, be produced according to Mr. Beck's theory, in that particular slide the beads are grouped in *grape-like* bunches on a firm thread. The threads of both species are perfectly dry, and not viscid between the beads. If the web of an *Epeira* is caught on a slip of glass its form is entirely destroyed; the fluid viscid drops being as it were blotted out, whilst in the second kind of web the threads, with their harder grape-like bunches, will remain distinct and uninjured.

In *Epeira* the four external spinnerets are arranged in two pairs, each pair containing tubuli differing from those of the other, so that though the glands of these two pairs of spinnerets are similar, it seems reasonable to suppose that they are used for different purposes.

Captain Lang thinks that but one pair of these exterior spinnerets is employed in forming the concentric line, which is varnished over, as it runs out, by the third interior pair of spinnerets, furnished with a viscid secretion from a totally different set of glands. There are other offices for which the other pair of exterior spinnerets with their different tubuli may be needed, and also where no viscidity of thread may be required, as in the formation of the radial lines, which are thicker and more elastic; or for that of the web with which the insect victim is swathed in a mummy-like shroud.

From slides showing the attachments of the *Epeira's* radial thread it would appear that the spider uses its spinnerets as a painter does his brush; the very delicate threads issuing from each tubule being dashed against the surface, formed into an entangled mass of loops, and then drawn out into one compound, though practically simple thread.

* Report supplied by Mr. B. J. Austin.

Further proof of the reasonableness of Captain Lang's explanation was afforded by the fact that when the web is tightly stretched without touching the glass the thread may be seen, with a $\frac{4}{10}$ object-glass, *running through* the viscid beads, which appear as if transparent and strung on a thread. This would not be the case in a *single* viscid line running by *molecular attraction* into beads or drops. The conclusion, therefore, seems inevitable that as it is only the spiral or concentric lines which are strung with these beads, they must be furnished from the secretion proceeding from the single inner pair of spinnerets; the glands differing so materially from those of the two outer pairs which have so much more work to perform.

Quekett Microscopical Club.

Ordinary Meeting, October 23.—Dr. Matthews, F.R.M.S., President, in the chair.

Mr. R. Packenham Williams read a paper "On Cutting Sections of the Eyes of Insects, and on a New Instrument for that purpose." The method of preparing the head for cutting was first described; the most successful plan being first to shake the insect gently in a phial of benzine—then to soak it in alcohol 60° over proof for a time varying from four to forty-eight hours—this was considered to be the most difficult part of the operation, some specimens becoming hard sooner than others; and it was suggested that the best preparations might possibly be made from insects just on the point of emergence from the chrysalis. The head, after being hardened, was to be imbedded in a mixture of butter of cocoa and bleached beeswax, with the addition of a little new Canada balsam. This compound melted at about 120°. The head was to be placed in the wax so that the cut should be at right angles to the chord of the segment forming the outline of the eye, the most satisfactory section being that in such a direction as to show the structure of both eyes. The cutter was to be wetted with spirit of turpentine, and one cut having been made, a little wax of a lower melting point was applied to the cut surface, so that the next section might be supported by a thin film of wax, and the cavities of the head were also to be filled with wax so as to give more effectual support. For the same purpose a piece of tissue paper laid on the face of the section was often advantageous. The wax was to be removed by warming gently in turpentine, and the specimen could then be mounted in *new* balsam.

The instrument used was then minutely described. This consisted essentially of a rotating circular cutter, and a contrivance similar to the slide-rest of a lathe, by means of which the thickness and direction of the sections could be adjusted, and the object advanced against the cutter while rotating. The cutter was extremely thin, and moved with great accuracy in an exact plane. This was possible, because it could be ground, polished, and sharpened on the pivots and in the position which it would permanently occupy. The slide regulating the thickness of the section could be adjusted to the $\frac{1}{10000}$ of an inch, while that which advanced the object against the cutter

moved $\frac{1}{30}$ of an inch to three revolutions of the cutter, and, as the latter was nearly $\frac{1}{3}$ of an inch in diameter, this was equivalent to a straight draw of three inches for every $\frac{1}{30}$ of an inch cut: this relation was attained mechanically. Great speed was not recommended. The object was supported on a little ebonite block capable of motion round an axis for the adjustment of the object to be cut in a vertical plane. The machine was of very diminutive size and delicate construction.

A paper was read by Dr. D. Moore "On the Generative Processes of the Cockle (*Cardium edule*), Mussel (*Mytilus edulis*), and the Oyster (*Ostrea edulis*)." He first drew attention to the difference of opinion on the subject, by contrasting Professor Owen's statement of twenty years ago with Professor Rolleston's more recent statement in 1870; Professor Owen asserting that all *Lamellibranchs* had the sexes in distinct individuals, Professor Rolleston excluding *Ostrea* and *Cyclas*, which had the sexes united in one individual. He stated that his observations had led him to the conclusion that the cockle and the mussel were also truly hermaphrodite, having the sexes united in one individual. He then proceeded to give an outline of the minute anatomy and general distribution of the generative gland in the cockle, mussel, and oyster, stating that all the steps from a gland containing immature sperm cells to one containing perfect eggs in the oyster, and eggs and young in the cockle and mussel, could be clearly traced—the glands containing spermatozoa being only a stage in the history of the gland containing eggs or young. He then drew attention to some diagrams enlarged from camera-lucida drawings, showing three principal stages in the history of the gland:—1st, when the gland contained imperfect spermatozoa; 2nd, when it contained perfect spermatozoa and clear cells, the entrance of the spermatozoa into the clear cells, which he had observed in the cockle, constituting the impregnation, which led to the 3rd stage of well-formed and easily recognizable eggs. In the case of the cockle and mussel, the eggs were hatched inside the animal, and the young were brought to maturity in a system of tubes, which were much more developed in the cockle than in the mussel. In these tubes the young were found in all stages of growth, with a number of yelk balls which doubtless supplied them with nourishment. The young were figured in the diagrams, special attention being directed to the stage at which they were extruded from the parent, when they constituted a true larval form, and possessed a rudimentary shell. In the oyster the eggs were extruded from the generative gland into the buccal pouch, between the palpi and the layers of the branchiæ, where they remained surrounded by a gelatinous substance, until they were developed into freely moving ciliated young, when they were puffed out from the parent shell a small number at a time. Dr. Moore concluded by stating some general considerations which he thought pointed in the same direction as his observations on the generative processes in these animals.

This paper was further illustrated by various preparations, which were exhibited at the close of the meeting.

INDEX TO VOLUME XII.